

Who will Mourn for the SST?

THE end of the SST project is an important happening. Of that there should be no doubt. It is not often that people, voters and their elected representatives, play such a decisive part in strictly technical decisions. Who will now say that modern society is powerless to put a rein on technical development? But in the nature of the aircraft business, with its international ramifications, there is no doubt that the American decision will cast a long shadow abroad. Is it now more likely or less likely that the Concorde project will be commercially viable? That is something for the British and French Governments to worry about in the next few weeks. It will be interesting to see how far the writ of Congress runs beyond the United States. More distantly, it will be important to work out better and less rancorous ways of making decisions like that arrived at last week, not merely on a national basis but internationally. In all the circumstances, no amount of concern for the ups and downs of the SST project can be regarded as superfluous.

The first thing to be said is that the decision to abandon the construction of two prototypes (see page 272) might have been avoided if the Administration had been less tactless and even inept. Obviously it would have helped the Administration's cause if it had taken the initiative in bringing out into the open questions about the possible damage that might be done by supersonic transport. In this, as in other projects, it would have been better to disarm the opposition with frankness than to have sought somehow to belittle it. The SST project might also have survived the Senate last week if its managers had shown some signs of willingness to compromise on the rate of development, and it is almost beyond belief that they should have been entirely innocent of plans for rescuing at least those parts of the SST programme which may be of lasting value. In the circumstances, it is understandable, if thoroughly illogical, that Congress should have voted to kill the SST while there seemed to be a chance. A year from now, or after the prototypes had taken to the air, the pressures to continue might have been irresistible. That is how many people will have argued. The trouble is that the halting of the SST programme at this stage may in the long run bring unwelcome side effects.

The truth is that nobody can seriously complain that the two prototypes would have seriously damaged the environment. Quite proper anxiety about the effects of fleets of supersonic aircraft on the stratosphere, or about the extra noise that these machines would provide in the neighbourhood of international airports, would have been relevant in the United States only to the question whether or not to go ahead with the manufacture of the aircraft. The issue last week was not whether supersonic aircraft would be an intolerable public nuisance but whether it would be worth spending a further \$500 million to complete a development programme which is already two-thirds of the way towards its goal. This is the kind of problem with which betting men should be familiar. At this stage, nobody can be sure whether supersonic air transport will be profitable, but there is a chance that continued development would provide the United States with a stake—presumably a very large stake—in a busi-

ness that could be worth \$2,000 million a year or more. Undoubtedly, profitability would eventually have been determined in important ways by the regulations in force in the late seventies about the noise intensities considered acceptable in the neighbourhood of airports. The commercial prospects of the SST might indeed have been entirely frustrated if it turns out that operation of supersonic fleets would damage the environment in unacceptable ways (in which case there will be some formidable international wrangles about the rights and wrongs of operating the Russian and Anglo-French supersonic aircraft). For all kinds of reasons, this is not a good time for making a judgment about the potential benefits and risks (in strictly commercial terms) of going ahead with the project. The depressed conditions of the airline business are the most serious difficulty. The need to know more about the economics of operating a supersonic transport system is another.

One danger in the situation which has now emerged is that it will encourage the confused belief in modern society that technology is not merely suspect but evil. Last month the otherwise moderate Mr William Ruckelshaus, the head of the United States Environmental Protection Agency, declared in ringing tones that "the mindless onrush of technology must be stopped". What about the mindless way in which people who should know better turn their backs on recent economic history and fail to acknowledge the social benefits which the discriminating use of technology has brought? What about the mindless way in which it is supposed that nations and governments are incapable of distinguishing and impotent at discriminating between desirable and undesirable applications of new techniques? Is there not a risk that the affair of the SST, confused though it is with such things as the decline in the fortunes of President Nixon, will be remembered as a great populist victory in the war of the people against technology?

Another consequence, in the United States at least, will be to sharpen the contradictions which already exist in the public sponsorship of gigantic technical projects. The SST was the first important aircraft in the United States to have been undertaken without a military subsidy. It is no surprise that a supersonic aircraft should be the first to have to stand on its own feet, for military aircraft require supersonic speed only in exceptional circumstances, not in sustained flight. In the debate in the past few weeks, several moderate opponents of the SST have argued that if the machine is potentially competitive with existing types of aircraft, the aircraft industry should foot the cost of development. This is logical enough, but it may be unrealistically stern. One obvious difficulty is that the scale of projects such as the SST development is too great for companies to shoulder on their own.

What then should be the relationship between governments and private industry in the financing of new technical developments, not merely supersonic aircraft but new types of nuclear power stations, electronic computers and the whole machinery of advanced technology? How should governments draw a line between the over-hasty support of premature or even misguided development and the temptation to postpone development until it is too



late? In the past few decades, chauvinism has too often been a guide. Programmes of research in nuclear power have proliferated because too many countries have been anxious not to be left behind in some non-existent race, with the result that such markets as there have been for new technical products have been hopelessly fragmented. One of the healthy undertones in the SST debate has been the tendency to say that the United States need not always follow the Russian example. That is a good starting point.

The strongest case against the SST, as against the Concorde and the Tu144, is that the airlines are not yet ready for it. The fact that these are dog days for the growth of air traffic, so that it may be hard for airlines to buy what they have ordered, is perhaps less important

than that the airline industry is quite inadequately structured to meet modern needs. Too many airlines are small and unprofitable. Too many airlines, large and small, are subsidized by governments. All of them belong to a cartel (the International Air Transport Association) which robs the operators of a chance to fix economic fares or to develop economic patterns of travel. With aircraft as with other products, there must be some general understanding that governments must not subsidize important technical developments for markets which are non-existent or artificially sustained. It would be a public service if, in the discussion there should now be of relationships between governments and industry, a code of proper practice were developed.

Turning Point for SALT

THE resumption of the SALT negotiations in Vienna seems to have brought to a head the anxieties of both sides about permanent commitments to restrict the uses made of strategic weapons. After three series of talks which appear chiefly to have been concerned with the definition of the problem, the United States and the Soviet Union seem now to be confronted with the need to say what kind of treaty on the limitation of strategic weapons they would be prepared to sign.

What kind of agreement is likely to emerge? The Soviet Union appears to have asked that a ban on the deployment of anti-ballistic missiles should come first in any list of items to be negotiated. As this proposal has been developed in the past few months, there are several pitfalls in the Russian version of such an agreement. To the United States, quite understandably, one of the chief objections to the Russian plan is that anti-missile defences would remain around Moscow and Washington, and that technical improvements would be permitted within these systems. The obvious danger is that it might be possible to develop within such zones of isolation anti-missile systems which have more than a merely local significance. But in any case, as all the primers on the theory of deterrence make plain, the protection of centres of population or even of governments tends to be destabilizing. If there is to be an agreement on the limitation of anti-missile systems of any kind, the only permissible weapons should be those that protect offensive missile emplacements, and which thus make it possible for potential victims of a first strike to sit tight in the knowledge that their capacity to retaliate will not be destroyed in the first few minutes. It is therefore to be hoped that the Soviet Union will settle for a simpler agreement—preferably one that does not raise all over again the issue of on-site inspection.

What has the United States to offer in return? For the past few months, President Nixon and Mr Melvin Laird have said quite openly that they will be satisfied only with an agreement covering offensive and defensive weapons. Presumably what they have in mind is an agreement under the terms of which the total number of missiles deployed by each of them would be kept within some predetermined limit. There has been a great deal of talk about the possibility of restricting the deployment of multiple re-entry vehicles, but only a little reflexion shows that such an agreement could not be verified. The American view that any agreement must mix together offensive and defensive weapons has a great deal to com-

mend it. Both sides should, however, recognize that in the long run, a stable system will be one in which there is some assurance that neither side can steal a march on the other by ingenious and secret technical development. This is why there is much promise in the news, apparent in the State Department document published earlier this week, that the United States plans later this year to bring up the comprehensive test ban agreement in the Geneva Disarmament Subcommittee. In the long run, it will be best for everybody if the SALT negotiations can be placed in that wider context.

100 Years Ago



A BILL TO ESTABLISH THE METRIC SYSTEM OF WEIGHTS AND MEASURES

THE following Bill, prepared and brought in by Mr. J. B. Smith, Sir Charles Adderley, Sir Thomas Bazley, Mr. Graves, Mr. Baines, Mr. Albert Pell, Mr. Muntz, and Mr. Dalglish, has been ordered to be printed by the House of Commons:—

Whereas it is desirable that the weights and measures of the United Kingdom should be decimalised, and made to correspond with those of other countries.

And whereas the use of metric weights and measures is now legal, but no provision has been made for procuring the standards of said metric weights and measures, and for verifying and stamping those in use under the said Act.

Be it enacted by the Queen's most excellent Majesty, by and with the advice and consent of the Lords Spiritual and Temporal, and Commons in this present Parliament assembled, and by the authority of the same as follows:—

1. From and after the expiration of () years after the passing of this Act, the length of the metre to be prepared under the authority of the Privy Council for Trade, verified by comparison with the original standard in Paris, having the words and figures "Standard Metre, 1871," engraved upon it, and kept in the custody of the Warden of the Standards, shall be and is hereby declared to be the unit or only standard measure of lineal extension, wherefrom or whereby all other measures of extension whatsoever, whether the same be lineal, superficial or of capacity, shall be derived, computed, and ascertained, and all such measures shall be taken in decimal multiples or decimal parts of their respective units.

From Nature, 3, 448, April 6, 1871

OLD WORLD

SPACE POLICY

Black Arrow Inadequate

BRITAIN'S space programme, which aims to use the Black Arrow launch vehicle to put one small satellite in a low orbit late this year, and to follow with a second test satellite sometime in 1974, looks inadequate to provide the experience of satellite technology which will enable British electronics companies to be competitive in the growing and prospectively highly profitable communications market. In a memorandum presented to Subcommittee B of the Select Committee on Science and Technology, the GEC-Marconi company, which dominates Britain's commercial space activities, claims that in order to obtain the necessary information about geostationary communications satellites to make their products commercially desirable, the company must have the experience which can only be obtained by placing test satellites in a geostationary orbit. Black Arrow is inadequate for this purpose, and the alternatives would seem to be either to buy American launch vehicles "off the shelf" or to rejoin the ELDO project.

The argument that Britain should partake in any satellite project depends on the assumption that potential users of communications satellites will not be happy to supply a contract for the installation of the large ground based parts of the system to a company which has no satellite capability. In the past, this has not been the case, as is emphasized by the impressive record of GEC-Marconi in providing ground stations in many parts of the world. But this situation is unlikely to continue as the market becomes more competitive. The company believes that only two carefully designed launches of large satellites into geostationary orbit could suffice to develop the required technology within five years; but the vehicle needed for these launches would probably be the very expensive Thor-Delta.

Industrial sources are not alone in questioning the wisdom of the planned Black Arrow programme, which is now being reviewed on a cost effective basis by the Ministry of Aviation Supply. Other policies which might well prove more efficient are to step up the rate of launchings, retaining Black Arrow, or to buy a relatively cheap American rocket, such as Scout, in either case aiming to solve the problems of satellite development using several small test satellites rather than the two large ones recommended by GEC-Marconi. But in the context of cost effectiveness, it is a sobering thought that only about half as much is being spent on Britain's total space effort as on Concorde, and that

military expenditure accounts for two-thirds of this total space budget.

So it seems that once again the whole future of Britain's role in the aerospace markets of the last quarter of the twentieth century must be rethought. If the cost of large American launchers really is prohibitive (or if there is any reason to believe that these may not be made available for work competing directly with that of companies in the United States) then Britain must once again embark on a collaborative venture, presumably by rejoining ELDO.

GOVERNMENT

Cottrell Steps Up

THE new chief scientific adviser to the Government is Sir Alan Cottrell, FRS. He succeeds Sir Solly Zuckerman, the zoologist and anatomist, who held the post since 1966 and who retired on March 31. Sir Alan's new job is the logical step forward in a succession of high level advisory roles which he has held since 1965 when he was appointed deputy chief scientific adviser (studies) to the Ministry of Defence. He was promoted to chief adviser in 1967 but soon moved to the Cabinet Office where he has been deputy chief scientific adviser to the Government since 1968.

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[Photo: Camera Press Ltd
Sir Alan Cottrell]

Sir Alan, 52, was deputy head of the Metallurgy Division at the Atomic Energy Research Establishment, Harwell, from 1955-58, and Goldsmiths' Professor of Metallurgy at Christ's College, Cambridge, from 1958-65. His deputy has not yet been named.

WATER RESOURCES

Desalination Reviewed

A REPORT by a Science Research Council working party has recommended that the money provided by the council for desalination research

should be approximately doubled by 1974 and that there should be some restructuring of research effort (*Desalination*, Science Research Council; 1971). The cost of the seven research programmes which the council supports at present is only about £125,000 but extensive government sponsored work is also in progress at several Atomic Energy Research Establishments.

Even though the United Kingdom has abundant rainfall compared with many countries, the predicted doubling of domestic water consumption to more than 70,000 gallons per year per head by the end of the century almost certainly means that desalinated water will account for a significant proportion of the total demand. But at present British interests tend to be directed towards export sales and world installed desalination capacity is increasing at almost 25 per cent per year.

The report suggests that there should be certain shifts of emphasis so that extra funds can be used as efficiently as possible. In particular, it recommends that research into desalination by distillation should be maintained at about the same level over the next few years, because it is "well founded and adequate"; it is widely felt that distillation research could lead to efficiency improvements of about 20 per cent. Glasgow, Heriot-Watt and Strathclyde Universities seem to have attracted the largest SRC grants in this field.

The development of membranes for osmosis, dialysis and so on has been slow and, on the whole, unsuccessful and the working party considers that the approach has been too fragmentary. It therefore advocates the launching of a large multidisciplinary programme, the goal of which would be a complete system operating on the reverse osmosis principle. Such a project would be supplemented by a number of supporting projects dealing with more fundamental problems, such as the modification of the surface properties of membranes with time and the adhesion of synthetic materials under water.

A frequently used type of membrane comprises a secondary cellulose acetate desalinating film (<1 µm thick) mounted on a thicker and much more porous backing of the same material. A commercially viable membrane should ideally be capable of withstanding a pressure difference of about 100 atmospheres, have a lifetime of at least 2 years and be capable of single stage desalination of seawater as well as brackish river water. Although the membranes developed so far fall far short of these requirements, there are clearly great advances to be made in desalination if future work is successful.

As far as desalination by freezing is concerned, the working party finds that there is real research potential but

inadequate funding. The SRC is urged to consider favourably proposals such as the nucleation of ice and the growth of ice crystals in salt water, which are being investigated to a limited extent in one or two places, notably the University of Strathclyde.

The SRC is responsible for the financing of university desalination projects, but the Atomic Energy Authority has been intimately involved in two programmes of research and development sponsored by the government since 1965. The AEA was the obvious choice for the centrepin of a national effort because it possessed so much of the expertise appropriate to desalination (for example, materials science and heat transfer technology). The AEA embarked on its second three-year programme in 1968 (estimated cost £4 million) and has accrued considerable experience of the three principal desalination processes.

Although desalination research is quite lively in the United Kingdom, expenditure is still small compared with other countries such as the United States, where spending is running at a level of about £8 million per year. The strong position of the United Kingdom in export markets for desalination equipment is also being increasingly challenged by Japan where research expenditure is likely to be more than £12 million during the next six to seven years.

ECOLOGICAL

Understanding the Tay

RESEARCH aimed at building up a complete picture of the Tay estuary in Scotland is being carried out at the University of Dundee by a multi-disciplinary group at the Tay Estuary Research Centre. Many of the efforts to quantify the estuarine environment could make a useful contribution to the choice of new industrial sites in the area through recommendations about the quantity of effluent which could safely be discharged into the estuary and, perhaps more important, the places at which discharges should take place.

The research centre was set up in 1964 with a grant from the old Department of Scientific and Industrial Research (DSIR) and has since been funded principally by the Natural Environment Research Council to the tune of about £30,000 and by the University of Dundee itself and others. In the early days of the centre, most of the research was concerned with the deposition of sediments, but since 1967 the departments of civil engineering, biological sciences and mathematics have joined the geology department in establishing a truly interdisciplinary approach to the problems. The depart-

ments of bacteriology and geography have also contributed extensively to the work in the past few years and external bodies such as the Tay River Purification Board have both gained and given information vital to the continuing existence of the Tay as one of the cleanest rivers in the United Kingdom, as well as one of the biggest.

Much of the work undertaken by the civil engineers and mathematicians is concerned with investigations of the progress of the tidal wave up the estuary, and the effects of the height of the tide and the magnitude of the river flow on salinity values at various positions in the stream. This has facilitated two and three dimensional modelling of the Tay estuary, which will be useful for deciding on sites for any future industrial effluent emissions. A related estuarine parameter which can be monitored with fairly simple apparatus is the concentration of suspended silt; the regular variations of this quantity give more clues to the effect of complex circulation behaviour on the overall environment of the river. The rather more difficult measurements of concentrations of dissolved oxygen and the bacterium *Escherichia coli* have led to a preliminary mathematical model from which variations of concentration at any point can be deduced.

As far as geology is concerned, the research centre has been chiefly engaged in studies of sediment behaviour, especially deposition behaviour and movement of the sandbanks exposed at low tide. These are of particular importance to the Dundee Harbour Trust for future planning of deep water inlets to the Dundee docks. Fluorescent tracer work shows, for example, that during the past thirty years or so there has been a considerable accretion of material at the mouth of the estuary which originates in nearby St Andrews Bay; the exact rates and nature of the filling process may well be important because of the possible effects on river and tidal flow rates and hence on the other quantities which are measured higher up the estuary.

A great deal of the biological research has been carried out to determine the effect of agricultural fertilizers and urban effluents on the production of microorganisms in the Tay valley and of phosphorus input into both the river Tay and the sixty-eight lochs within the estuary catchment area. One interesting conclusion of the research is that the residence time for pollutants in the rivers and estuary is about three weeks on average, although this can be very variable, depending on the rate of flow.

The work of the research centre is also highly relevant to the Tay Development Plan, drawn up by

economists at the University of Dundee, which suggests ways in which the industrial potential of the Tay area could be tapped by a two-fold increase in population based chiefly on the existing conurbations.

Parliament in Britain

RB211

QUESTIONS about the future of the unhappy RB211 project show no signs of abating. Mr Frederick Corfield, Minister of Aviation Supply, in reply to queries from Mr Phillip Whitehead, explained that Lockheed is aware that research on the engine is still in progress and at such a level that the project will be able to go ahead without further delay if a new contract is negotiated. The present negotiations are concerned with the broader aspects of a new contract for the supply of the RB211 engine; these engines would be fitted with titanium fan blades, but this does not rule out the later use of 'Hyfil' carbon fibre blades. In reply to a question from Mr Leslie Huckfield, Mr Corfield said that no delay had been caused to the Lockheed TriStar certification schedule by late delivery of flight-rated RB211 engines. The first and second TriStars flew on time. (Written answers, March 24.)

Communications

MR CHRISTOPHER CHATAWAY, Minister of Posts and Telecommunications, said in answer to a question from Mr Laurance Reed that over 500 British communications cables lie submerged on the British continental shelf. Of these, 70 lie in open sea areas, and it is the Post Office which carries the primary responsibility for ensuring that the cables are protected against the ravages of fishing gear and industrial operations on the sea bed, and for repairing faults. (Written answers, March 26.)

Agricultural Research Council

OF the total staff employed by the Agricultural Research Council, just over 17 per cent are classed as scientific staff according to figures given by Mrs Margaret Thatcher, Secretary of State for Education and Science, in reply to a question from Mr Merlyn Rees. The staff distribution by categories at March 1, 1971 was:

Scientific class	335
Experimental class and scientific assistants	699
Technical and other staff	170
Administrative staff	296
Industrial staff	424
Total	1,924

(Written answer, March 24.)

NEW WORLD

Playing it Cool at the NSF

from our Special Correspondent

Washington, March

THE National Science Foundation seems to have learned in the past year how to go discreetly about its business, presenting what is called a low profile to the outside world and staying out of trouble that might prejudice its new-found prosperity. One result is that the public inquiry last week into the foundation's budget for the coming year was a bland contrast with the stirring occasions a year ago when it seemed for a time as if the director of the foundation, Dr William McElroy, and the then chairman of the National Science Board, Dr Philip Handler, were if necessary prepared to go down with the ship of graduate education. This year, the ship is said to be sinking, but Dr McElroy is full of urbane reasons why this does not matter.

The root of the foundation's new breeziness is the increase of its budget for 1972 (the financial year which begins on July 1, 1971) to \$619 million, compared with \$495 million in the current financial year and a mere \$460 million a year ago. Dr McElroy told the Subcommittee on Science, Research and Development in the House of Representatives last week that within its larger budget, the foundation plans to increase expenditure on research grants (from \$176 million to \$258 million), to "increase and concentrate scientific resources on specific societal problems" and to spend less—"we have realigned our priorities"—on the support of university departments and the students who attend them.

In part, the increased expenditure on research grants has been forced on the foundation by the abandonment of basic research projects by other agencies, largely as a result of the Mansfield Amendment. The foundation estimates that research projects worth \$58 million have been discontinued in the past eighteen months, and Dr McElroy expects further demands on his resources amounting to \$36 million to become apparent before the end of June. On the face of things, at least, it looks as if half the foundation's budget increase must be attributable to projects orphaned by the mission agencies.

The foundation has inherited actual laboratories as well as obligations from the mission agencies. Everybody seems to be cheerful about the transfer of the Magnet Laboratory at MIT from the Air Force to the National Science Foundation. There seem to be more

obvious difficulties with the transfer of the dozen or so interdisciplinary laboratories from the Advanced Research Projects Agency within the past year, and Dr McElroy said last week that he has started a survey to decide "how best to integrate the laboratories into the NSF".

Between them, the laboratories are responsible for about 30 per cent of the basic research in materials science carried out in the United States. One obvious difficulty is that their growth has in the past been guided by external demands from the mission agencies. The question is whether the foundation can set up as an arbiter of what kinds of research the laboratories should carry out. Another difficulty is that the laboratories have been used to living on block grants, not project grants of the kind which the foundation usually dispenses.

Within the pattern of research grant expenditure, the foundation plans to pay special attention to the social sciences. According to Dr Edward Creutz, Assistant Director for Research at the foundation, "we intend to expand our support in this field as rapidly as is consistent with maintaining high standards of workmanship". Dr Creutz singles out techniques for the measurement of social change and economic studies of productivity. In practice, the foundation's support for the social sciences increased from \$15 million last year to \$17 million in the current year, and is now expected to amount to \$27 million in 1972.

The pattern of research expenditure is shown in the Table on page 272. Riding on its record of success during the solar eclipse of 1970, the foundation is already making plans for the seven-minute eclipse expected in North Africa on June 30, 1973, and solar-terrestrial studies will use \$3.3 million in 1972. The prospectuses for research in the Earth sciences and oceanography pay close attention to questions such as the chemical oceanography of heavy metals (such as mercury), but there is every prospect that the productive seabed drilling of the past few months will forge ahead.

The scale of the foundation's research grant support for the biological sciences will be something of a surprise, and one of the questions left unexplored in last week's inquiry is the relationship between the NSF and the National Institutes of Health in the provision of research grants. (The

NIH are budgeting for \$700 million for research grants during 1972.) Cell biology will increase (from \$9 million to \$13.8 million) even more rapidly than the more fashionable ecological studies. The very large increase of the support for physics reflects the way in which the foundation has taken over from the Atomic Energy Commission responsibility for the preparations by university departments to participate in the work of the National Accelerator Laboratory, as well as for such things as the conversion of the Nevis accelerator at Columbia into a meson factory.

The foundation's concern for relevance, which last year flowered as the programme known as Interdisciplinary Research Relevant to Problems of Our Society (IRPOS), this year takes the form of Research Applied to National Needs (RANN). This organization (which will spend \$81 million next year on work that will have cost \$34 million in the current year) is an amalgam of the IRPOS schemes with some of the applied research (such as earthquake engineering) in which the foundation has interested itself in the past few years.

Among the new schemes on which the foundation will concentrate in 1972 are a number of regional environment studies (some of which were begun last year), the construction of a resource model of the Tennessee Valley, an environmental study of Chesapeake Bay, and some tentative studies of the application of scientific techniques to municipal management and to the study of energy resources.

The argument about the foundation's "realignment of priorities" in its educational work was genteel to the point of obscurity at the hearings last week. The rub comes in two ways—in the reduction of the foundation's direct expenditure on student grants and the like, and in the reduction of support for institutional development. Strictly educational work (involving summer schools for teachers, student grants and curriculum development) will be reduced from \$100 million to \$77 million. But there is also to be a reduction of institutional support for science (chiefly as block grants to departments) from \$34.5 million in

Our Washington Correspondent, who is on vacation, will have returned next week.

NSF Research Project Support

	Actual 1970 \$	Estimate 1971 \$	Estimate 1972 \$
Atmospheric sciences	7.9	9.2	11.9
Earth sciences	7.8	8.0	10.0
Oceanography	8.9	10.0	15.0
Biological sciences	40.9	43.5	57.8
Physics	28.2	30.8	43.0
Chemistry	17.4	19.5	27.7
Astronomy	5.8	6.4	8.6
Mathematics	12.7	13.4	15.9
Social sciences	15.4	17.4	27.5
Engineering	16.7	17.7	27.6
Materials research laboratories	—	—	12.8
Research facilities and equipment	6.5	5.8	5.8
International Biological Programme	4.0	7.0	10.0
Global Atmospheric Research Programme	1.5	2.0	2.5
Total	173.7	190.7	276.1

Figures are expressed in millions of dollars.

the current year (compared with \$44 million last year) to \$12 million in 1972.

As foreseen a year ago, the foundation's spending on graduate traineeships will be dispensed with altogether as present holders of these stipends graduate. Fellowships will remain, and will be awarded at the rate of about 500 a year to those graduating students from undergraduate schools who are successful in the annual competition. At the same time, postdoctoral fellowships will be eliminated. The scheme introduced two years ago for encouraging the involvement of undergraduates in research projects mounted in their universities has now been replaced by a scheme costing \$2 million a year for allowing them to carry out pieces of research under their own steam.

The foundation's defence of its reduced educational programme takes several forms. On the disappearance

of its tuitional support, for example, Dr McElroy said that this support had in the past been devoted to roughly 100 universities in which it had been decided there was room for strengthening of new graduate schools. But now, he said, these institutions can "compete effectively" with others, and that the time had come when continuing support should be given only to "young investigators applying for research grants".

On the attenuation of the traineeship programme, both Dr Humphreys and Dr McElroy took the line that there are now signs that the supply of PhDs and the demand for them are more nearly in balance than at any time in the past decade, but the force of this argument would have been greater if Dr Humphreys had not found himself having to describe the abandonment of the National Register of Scientific and Technical Personnel, by means of

which an attempt has been made to follow the careers of all technically trained people by means of questionnaires sent out every two years.

The second line of defence, also deployed last week, is that the foundation's provision of traineeships is in any case not directly relevant to the recruitment to graduate schools. Dr McElroy pointed out last week that recruitment had actually increased by 4.8 per cent last autumn, although there have evidently been sharp reductions in subjects such as physics.

If complaints about the foundation's educational programmes were muffled at the hearings last week, those there might have been about the provision of facilities for radio astronomy were even more discreet. The resurfacing of the Arecibo telescope will go ahead, but there is no new instrument over the horizon. At the same time, there is a cheerful growth of the foundation's support for institutions such as the Kitt Peak Observatory.

What will the committee have made of all this? On the face of things, it will take some time before the committee shakes down under its new chairman, Representative John W. Davis of Georgia. With the disappearance of Mr Daddario, for whom the subcommittee became almost a personal hobby, it seems as if the evident sharpness of the new committee members needs some organizing focus. Another conspicuous change is that attendance at committee hearings is now fuller than ever. This no doubt is a simple function of the way in which the National Science Foundation has managed to emerge from the budget allocations as one of the most rapidly growing of all government agencies.

Insubstantial Legacy of the SST

from our Special Correspondent

Washington, March 22

WHATEVER may be the future of supersonic air travel, the ending of government support for the two SST prototypes marks a turning point in the relationship between Congress and technology. For the time being, at least, social utility will be the essential criterion for public sponsorship of non-military projects. It is also clear that the vote by the Senate last Wednesday against the continuation of the development programme was determined not merely by technical and economic considerations but by political considerations as well. These are unhappy times for President Nixon, and the Administration's defeat in the Senate in the event by the unexpectedly comfortable majority of 51 to 46 will make them even unhappier.

The immediate future remains unclear. Funds for phase III of the SST programme run out tomorrow night (Tuesday). Mr William Magruder, Director of Supersonic Transport Development at the Department of Transportation, has failed in attempts to raise money on Wall Street. Rumours that a Japanese consortium is prepared to pay ten per cent of the cost so far incurred by the US Government—\$864 million by tomorrow night—for the privilege of carrying on from where the programme now rests are intriguing but probably insubstantial. An outsider would have to be able to persuade the Boeing company and its workpeople, 7,000 of whom are now likely to lose their jobs, that he has funds enough to ensure that the SST would be put through its flight certification pro-

gramme and that manufacture could also be put in hand.

In the circumstances, the best hope, slender as hopes can ever be, is that there will be a government guarantee of a private loan to Boeing and General Electric, responsible respectively for the airframe and the engines. The snag, of course, is that the Government would find itself having to pay up if, for some reason, the SST were carried through its development programme but not put into production. When even those who voted last week for continuing with the SST insisted that development should not automatically presage manufacture, it is plain that this would be an unpalatable expedient. Disentangling the partnership between the Department of Transportation and its contractors is likely to be difficult.

Under the SST contract, cancellation by the Government entails that the Department of Transportation should pay the contractors for their development costs so far, a total of \$85 million, as well as for the cost of breaking the contract at short notice, perhaps as much as \$10 million (see Table). The US Treasury will also be required to return the \$22 million it holds on deposit from airlines which have reserved positions in the SST queue—a prospect that will no doubt cheer up a good many of the airlines which are at present hard-pressed for cash, although this has nothing to do with Congress.

Costs of SST Programme to Completion of Prototypes (Phases I, II and III)

	Govern- ment	Con- tractors	Air- lines	Total
	\$	\$	\$	\$
Spent so far	864	85	59	1,008
Extra required	478	105	0	583
Total	1,342	190	59	1,591

Figures are given in millions of dollars.

It is less certain what will happen to the contributions the airlines have made to the cost of the development programme, \$59 million so far. The intention has been that airlines contributing towards the development cost (as those wishing to reserve places in the queue have been required to do since June 1967) should recover their investment and half as much again by a preferred royalty on sales. The SST office appears to feel a moral obligation to repay this cost, but it is not clear how Congress will react. In any case, the net cost of cancellation will be either \$97 million (including some administrative overhead) or \$157 million. In short, by the time the Government buys its way out of the contract, its entire expenditure will amount to 76 per cent of the public contributions to the development programme as a whole. Given that the contractors will no doubt be smarting from also having spent a quite unrecoverable \$79 million on plant and interest charges, it is clear that Congress has been indifferent to strictly monetary considerations as well as to the relationship between the Administration and commercial industry.

The costs of the later stages of the SST programme have not recently been defined with anything like precision, which no doubt has contributed to the sense in the past two weeks that the Administration has been trying to persuade Congress from one pig in a poke to the next. From the beginning of the enterprise, however, the Federal Government has been insisting that it would be for the manufacturers and their customers to finance the programme of flight testing (estimated to cost \$800 million at 1967 prices and to begin in mid-1972) as well as the invest-

ment in manufacturing facilities and work in progress (estimated to amount to between \$2,000 and \$2,500 million by the time that certification would have been complete, early in 1978). This promise would have been believed more easily if the Boeing company had not on two occasions postponed the production of a scheme for the private financing of the manufacturing programme.

What has the Boeing company and its associates to show for all the money spent so far? The most conspicuous relic of the SST programme is the mock-up in the Seattle plant which was completed in June last year and which has since been elaborated and refined. The manufacture of parts for the airframes of the first two prototypes has been under way since then—by now, the landing gear forgings should have been machined. The design of the wing was still in doubt last week, chiefly because wind tunnel tests have shown that the original design is subject to flutter, and there is talk of strengthening the structure or of increasing the outboard mass of the wing by means of suspended weights (which in any case will eat into the lifting capacity of the machine). There seems to be broad agreement that it was wise to have thought of fitting the SST with a tail (in which respect the design differs from that of the Concorde and the Tu144), chiefly because this simplifies the problems of attitude control and, in particular, makes it possible to provide lifting flaps at take-off and landing). The penalty is increased drag at cruising speed.

The engines of the SST, now intended to produce 68,800 pounds of thrust, are being built and the first of them has been tested at full power. Altogether, the cancellation of the programme means that eight preliminary engines and bits and pieces for a further twelve will be left on the shelf. In the quest for quieter engines, the diameter (and thus the mass flow) has been increased.

Only a few years ago, the sheer technical interest of the SST programme would have commanded the support and even the enthusiasm of Congress, but last week in the Senate debate there were hardly any echoes of this theme. Those who have supported the project in the Senate and, a week earlier, in the House of Representatives, have tended rather to rely on the argument that failure to push ahead with the development would imply that United States airlines would be compelled either to buy Anglo-French—possibly even Russian—aircraft or to relinquish a lucrative part of a growing air traffic industry to competitors abroad.

Senator Barry Goldwater, for example, said in the debate last week: "There will be SSTs . . . the only question is whether they will be American

SSTs or British-French SSTs or Russian SSTs". Characteristically, Senator William Fulbright pointed out that there is much to be said in aircraft engineering for letting the pioneers make mistakes, as with the development of civil jets. "Who got the business? It was not the British with the Comet; it was Boeing, with the 707."

The tangible version of this argument is the effect which the abandonment of the SST (and the supposed success of the Concorde) would have on the United States balance of payments. The Administration has consistently taken the line that abandoning the project would mean a loss of \$22,000 million between 1978 and 1990—\$10,000 million in lost exports and \$12,000 million in the purchase of supersonic transports from Europe or the Soviet Union.

Senator Muskie's speech, which set the tone of the debate last week, has the interest of showing where a likely presidential candidate stands on issues such as that of the SST. He emerged as a moderate opponent. He said that the "wastes that it will inject into the upper atmosphere could cause sweeping damage to the world's environment", that sonic booms could cause "ecological harm" and that the engines of aircraft on the ground would barely conform with statutory limits which may in any case be dangerously slack. And in any case, the market for which the SST has been designed may well disappear under the weight of the surcharges that will be necessary to make supersonic aircraft profitable.

On international competition, Mr Muskie said that "another nation's squandering of precious resources is a good argument for doing the same thing here", that the number of jobs involved (85,000 at the peak of the programme three or four years from now) was small compared with the five million people already out of work. (This has not prevented Senator Muskie from introducing a bill to provide \$100 million for development in air traffic control and such projects to help with unemployment in Seattle.) And, he said, there are plenty of useful projects in transport engineering, from the reduction of aircraft noise and the development of VTOL and STOL machines to the design of mass transportation systems, on which governments could more usefully spend their money.

Among other arguments against the SST programme was that of Senator Scott, who proclaimed that if there was a case for supporting with public funds technical development in the aircraft industry so as to maintain an internationally dominant position, then there might also be a need for public support in the heavy electrical industry and in other fields as well.

In all the circumstances, one of the

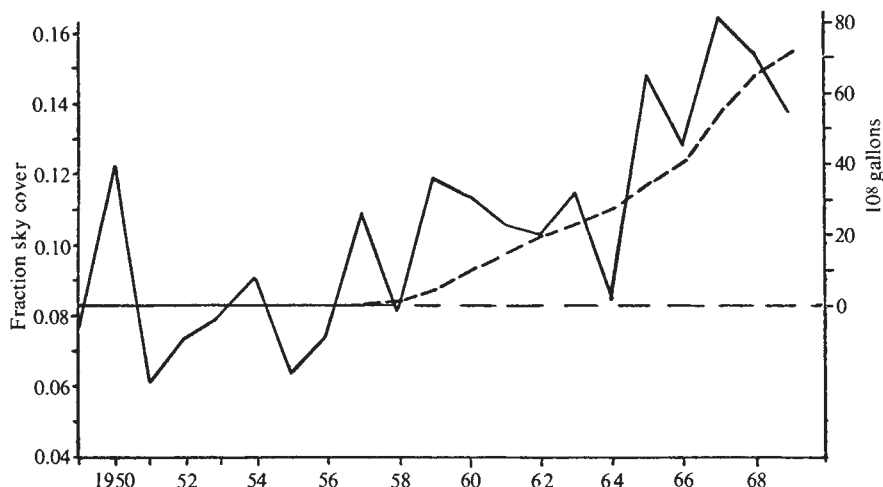


Fig 1. Change of upper tropospheric cloudiness above Salt Lake City (fraction of sky cover, left hand scale) and consumption of jet fuel (in units of 10^8 gallons, dotted line, right hand scale) as a function of time. (Source: T. Carpenter, *Met. Statistics*, ESSA.)

most puzzling aspects of the past few weeks has been the way in which the Administration has resolutely stuck out for the whole cake or nothing—one of the most poignant speeches in the Senate was that of Senator Javits, who has consistently opposed federal support for the SST project but who pleaded last week for a slower programme that could be more easily supported by non-federal sources.

To the extent that the debate was not a rehearsal of previously prepared positions, it must be counted not merely as a triumph for the opponents of the SST but for what may be called the moderate view of the environmental issue. Much of this credit must go to the director of the new Environmental Protection Agency, Mr William Ruckelshaus, who has been able to command respect for the view that two prototype aircraft by themselves would be a negligible threat to the environment, and that the Administration is now better equipped than ever to call a halt to potentially harmful projects at the appropriate time.

One of the most valuable legacies of the SST may be the impetus which it has provided for a thorough investigation of the environmental consequences of these aircraft. A year ago, \$27 million was allocated to this work, and it seems clear that one important benefit will be a better understanding of the movement of air masses and of contaminants within the stratosphere.

One particular issue will no doubt be the chemistry of ozone in the stratosphere, if only because of the suggestion in the past few months (by Professor James W. Macdonald of the Institute of Atmospheric Sciences at Tucson) that reactions between ozone and water vapour from a fleet of SST aircraft would decrease the average concentration of ozone by between one and four

per cent, and would thus increase the flux of potentially carcinogenic radiation reaching people's skins. Dr Macdonald's argument that one result would be an increase of the incidence of skin cancer in the proportion of one part in 20,000 has not carried much weight, chiefly because the proportion is such a small fraction of other fluctuations in natural exposure to ultraviolet radiation.

What has, however, emerged from the long wrangle about the atmospheric effects of aircraft operations is that there appears already to have been a marked increase of cloudiness at the altitude of conventional jet aircraft above normally cloudless places such as Denver and Salt Lake City. Fig. 1 shows that cloudiness high in the troposphere above Salt Lake City has for practical purposes doubled in the decade since the introduction of jet aircraft. As yet, it is not known how widespread is this effect, while its consequences for the Earth's albedo are equally in doubt.

One of the surprises of the debate last week, that in the House of Representatives a week earlier and the evidence provided to the Congressional committees in the past two years is that there have been only the most rudimentary attempts to calculate the overall cost of operating a supersonic transport system, taking account of variables such as the turn-round time of aircraft on the ground as well as of the inevitable time that even supersonic aircraft would have to spend in queues waiting to land. So far as Congress is concerned, the fullest argument has been provided by Mr Russell Brown, a member of the coalition against the SST. The failure of the Administration to provide similarly detailed arguments may have helped to lose the case for the SST.

PESTICIDES

New Law in Prospect

THE campaign against persistent insecticides moved forward another step last week, when the Senate Committee on Agricultural Research and General Legislation began its inquiries into the Administration's proposal that all uses of chemical pesticides—a term which includes herbicides and rodenticides—should in future be individually licensed by a competent authority. The case for the new legislation was presented by Mr William Ruckelshaus, the director of the Environmental Protection Agency, who confessed that in spite of the research now under way in the Department of Agriculture, with the object of developing biological methods of controlling insect populations, chemical insecticides would probably be needed for "the foreseeable future".

The new bill will create three categories of insecticides—those which can be used freely, those allowed for restricted use and those which can be used "by permit only". Materials in the second category can be used only under the supervision of a sufficiently knowledgeable expert. Permits for the use of materials in the third category, largely reserved for persistent insecticides, will be issued by consultants licensed by state governments. The intention is that permits will be issued only when there is some assurance that the use of the insecticide concerned is at once safe and necessary.

One obvious difficulty in the introduction of the new legislation will be the need to train a new army of people qualified to superintend the application of restricted pesticides as well as to issue permits for the use of the more severely restricted materials. Because the burden of this work will fall in largely unpredictable ways on the shoulders of the state governments, it is understood that the new legislation will not come into effect until the training is complete. At that point, however, the United States will have the toughest pesticide legislation in existence.

In the meantime, the use of certain pesticides is being outlawed. On March 18, the Environmental Protection Agency announced the cancellation of aldrin and dieldrin—the result will probably be a series of appeals by the manufacturers that will allow use to continue for the best part of a year. The use of DDT in the United States has already stopped (although it is still permissible to manufacture for export) and there seems to be no prospect that it will be reintroduced even with the stringent controls specified in the new law. "I'm afraid that DDT has become the whipping boy—it's quite finished in this country" is how one official described the plight of the archetypal pesticide.

NEWS AND VIEWS

Evolution of New Species

IN the process of evolution, the formation of a new, incipient species is a crucial step. It happens when a group of individuals in a population becomes more or less reproductively isolated from the others. It is a crucial step because at this stage the group, now an incipient species, can continue to evolve in a direction of its own. As the outcrossing to individuals in the rest of the population becomes restricted, so the "gene flow" from them is reduced, and the group can evolve its own adaptations without genes from outside disrupting the process. The group can diverge further and further from the rest of the population, becoming a new species and eventually a new genus.

There have not been many experiments in which an incipient species has been produced in the laboratory. On page 289 of this issue of *Nature*, Dobzhansky and Pavlovsky report the results of some experiments in which they have managed to produce a new incipient species of *Drosophila paulistorum* from a stock that was originally part of a naturally occurring incipient species.

In plants, as Dobzhansky and Pavlovsky point out, the experimental production of new species is not so unusual. When an infertile hybrid is formed between two different species of plant, it can often be made fertile by doubling its number of chromosomes. The chromosomes can then pair normally. In this process the new species is produced by a special mechanism at a single step. It is a new species because the hybrids with the original parents are infertile. In nature, however, there are many examples of new species apparently in the process of gradual evolution. In different groups of the genus *Drosophila* there are very similar species which hardly ever interbreed and some which occasionally do. Such a group of species and incipient species has certainly not been produced by changes in the number of chromosomes. And indeed the evolution of new species must usually be a slow and gradual process.

Incipient species of plants have an easy way to prevent interbreeding: they can flower at different times. There is usually a great deal of genetic variability in the time of flowering which is therefore easily changed by selection. In animals the problem is one of producing sexual isolation between the groups. Sexual reproduction depends on the particular mating behaviour of the species. The evolution of incipient species therefore depends on the evolution of different mating behaviours. The incipient species can be said to be "ethologically isolated" from each other. Dobzhansky and Pavlovsky's present experiments demonstrate that this kind of isolation can be produced in the laboratory. Some earlier experiments by Koopman (*Evolution*, 4, 135; 1950) and Knight, Robertson and Waddington (*Evolution*, 10, 14; 1956) were also successful in producing some ethological isolation in *Drosophila*. The same basic technique was used in all these experiments. The two groups which are to be selected for isolation are made homozygous for different recessive mutations which can easily be recognized. Each group breeds true for its particular mutation. But if individuals from the two groups are crossed, their offspring get a wild type gene from one parent which is

dominant to the mutant gene they get from the other parent. Thus the hybrids are wild type and can be eliminated. The next generation is started only with mutant flies, which must therefore have been produced by matings of individuals within each group.

Koopman used two separate species of *Drosophila*, *D. pseudoobscura* and *D. persimilis*, and he was able to increase the isolation which was already present between them. Knight, Robertson and Waddington started their experiments with mutant strains of *D. melanogaster* that originally came from the same population. As their experiments proceeded, they found that the ratio of mutant flies to wild type flies in the progeny increased, thus showing that ethological isolation was gradually evolving. In the experiments now reported by Dobzhansky and Pavlovsky, the degree of the ethological isolation produced was carefully measured by observing the matings; it was found to have increased considerably. They started with a particular laboratory strain of *D. paulistorum*, the Llanos strain, so called because it was started by a female caught in the Llanos of Colombia. In *D. paulistorum* there are a number of incipient species (usually called "semispecies"). The Llanos strain was found to belong to a semispecies called Orinocan. Matings between Llanos and Orinocan flies were observed in a mating chamber. Almost as many matings took place between the Llanos and Orinocan strains as within them, so there was no isolation.

In the selection experiments, females of one strain were placed in bottles with males of both strains. A particular mutation, rough-eyed, was used to mark the Llanos strain. In all, six populations were set up using different recessive mutants to mark the Orinocan strain. After about sixty to seventy generations of selection, the ethological isolation was measured by observing the matings that took place. The degree of isolation Dobzhansky and Pavlovsky have produced by the selection against the hybrids is at least comparable to the isolation that exists in nature between the semispecies.

Of course selection for isolation is not likely to be as intense as this in nature. As in previous experiments for isolation, Dobzhansky and Pavlovsky removed all the hybrid offspring in setting up the population of the next generation. This is to make the hybrids completely inviable. But in nature when the process of divergence begins, the hybrids must be completely viable. They are likely to be more or less inviable only if they have been geographically isolated for a period and have evolved separately before coming together again. Then, as Dobzhansky and Pavlovsky's experiments show, the evolution of ethological isolation can take place.

But what of groups that have not been geographically separated? Can they become adapted to different environmental conditions and eventually split into two groups? Mayr, in his book *Animal Species and Evolution*, has argued that geographical isolation is almost always a necessary condition of the evolution of species. Against this view are the experiments of Thoday and Gibson (*Nature*, 193, 1164; 1962), who produced ethological isolation not by selecting against the hybrids

themselves but by selecting against individuals of *Drosophila melanogaster* who were intermediate in the number of their sternopleural bristles. In these experiments the flies used to set up the next generation were those with the highest and lowest number of bristles. This is called disruptive selection, which must occur in nature when a population is living in two or more different environments. The hybrids were thus disadvantageous to the extent that crosses between flies with high and low bristle numbers would tend to produce flies with intermediate bristle numbers. Eventually two groups with high and low bristle numbers were produced.

Thoday and Gibson's experiment produced isolation very rapidly but two other similar experiments have not done so. The rate of evolution, however, depends on the genetic variation available for selection. In characters like mating behaviour, the amount of variation probably differs greatly between lines. Dobzhansky and Pavlovsky's experiments produced considerable ethological isolation after about sixty generations. When isolation is selected for by the disruptive selection of other characters like bristle numbers, it will evolve much more slowly unless there is great genetic variation in mating behaviour.

Sex Chromosomes Turned Off

To a biologist faced with the choice of an experimental animal for fundamental studies of chromosome activity the kangaroo might not be quite the first species to spring to mind. But intriguing new knowledge of *X* chromosome activity in mammals has come from work on just such animals (see page 292 of this issue of *Nature*; *Nature*, **230**, 231; 1971 and *Nature New Biology*, **230**, 154 and 155; 1971).

Sharman, Richardson, Cooper and colleagues present evidence that in somatic cells of female kangaroos the *X* chromosome derived from the male parent is genetically inactive whereas that from the mother is active. It has been known for some time that only a single *X* is active in somatic cells of eutherian mammals (*Nature*, **190**, 372; 1961), and the phenomenon is regarded as a dosage compensation mechanism ensuring that chromosomally *XY* males and *XX* females have effectively the same dose of the products of *X*-linked genes. In eutherians, however, the inactive *X* may be either the maternal or paternal one in different cells of the same animal. A female inheriting two different *X*-linked genes from her two parents thus shows evidence of the presence of both. The peculiarity of the situation is that, whereas in the more usual diploid cases both genes act in each cell, in the female mammal there are two types of cell, one expressing maternal and the other paternal *X*-linked genes. If the cells of each type remain together in large clumps during development this may lead to macroscopically visible variegation, as in the tortoiseshell cat or various *X*-linked coat colour mutants of the mouse. More interesting, however, has been the demonstration at the cellular level that, if single-cell clones of cultured cells are derived from women heterozygous for *X*-linked genes determining enzyme variants, each clone shows the activity of only one of the two possible genes, whereas uncloned cultures derived from many cells shown both.

A presumed inactive *X* has been detected in all orders of eutherian mammals tested, and in marsupials, by means of the properties which it shares with other inactive chromatin, of late DNA replication (late labelling with ³H-thymidine) and condensation during interphase (to form the sex chromatin body against the nuclear membrane). The demonstration of actual genetic activity, however, depends on genetic markers, and these are only known for a few species. Tests of randomness depend either on genetic markers or on labelling studies with two recognizably different *X* chromosomes (see, for example, *Nature*, **206**, 900; 1965). By making use of

Ohno's hypothesis that the *X* chromosomes of all mammalian species carry the same genes, the Australian workers have succeeded in finding both these necessary conditions within a species, and have thus shown that in kangaroos the inactivation is not random.

This raises various questions. For example, is inactivation random in all eutherian mammals or will species be found in which it is not? There is still controversy as to whether paternal *X* inactivation is the rule in mules (*Nature*, **228**, 1321 and 1322; 1970). If so, is this the normal situation in the Equidae or is it a peculiarity resulting from a species cross? Given that inactivation is non-random, it is not surprising that it should be the paternal *X* which is inactive, because the *X*-*Y* chromosome pair is thought to become inactive during spermatogenesis. Conversely, in the female germ cell both *X* chromosomes are thought to be active. Thus, the random inactivation in eutherians, which takes place in early embryonic life, involves inactivation of the paternal and inactivation of the maternal *X* in some cells.

The most interesting question, of course, is the mechanism of *X* chromosome inactivation and the relation of this to chromosome activation in general. It is important to realize that this is a case of facultative inactivation. Chromatin, termed heterochromatin, which like the inactive *X* shows condensation and late replication, is now known to be of various types. One type, which remains virtually permanently in the heterochromatic state, and is therefore termed constitutive heterochromatin, is often found adjacent to centromeres, and centromeric heterochromatin of the mouse has been shown by Pardue and Gall (*Science*, **168**, 1357; 1970) and by Jones (*Nature*, **225**, 912; 1970) to contain satellite or repetitious DNA, the function of which is not yet known.

Cooper's model of the mechanism of *X* inactivation on page 292 will stimulate discussion but, as he points out, it raises at least as many difficulties as it solves. One surprising feature is his choice of a factor for inactivation rather than activation. Because it is clear from individuals with abnormal numbers of *X* chromosomes (*XO*, *XXY*, *XXXXY* and so on) that the phenomenon is that of single-*X* activity, it seems simpler to postulate an episome, activation centre, controlling element, or whatever, for activity rather than inactivity. But the finding of paternal *X* inactivation, together with paternal chromosome inactivation in mealy bugs and parental source effects in some of the other cases of facultative inactivation, strengthens the case for a model such as Cooper's.

NUCLEIC ACIDS

New Sequences

from our Molecular Biology Correspondent

RNA sequencing is now a growth industry. As in other such situations the products are of widely varying utility. An obvious sense of purpose informs the work on bacteriophage RNA, following on the momentous results from Sanger's laboratory on R17. One of the many important findings was that of Adams and Cory, namely that the tract of nucleotides, beginning at the 5' end, of which they sequenced the first seventy-four, does not code for any known protein of the phage. Ling (*Biochem. Biophys. Res. Commun.*, **42**, 82; 1971) has now examined the RNA of another phage of the same group, f2, and finds that the corresponding stretch of RNA is identical with that of R17. Fiers's group in Belgium have meanwhile been attacking the RNA of MS2, a third phage of the same family, with the same result (De Wachter *et al.*, *Proc. US Nat. Acad. Sci.*, **68**, 585; 1971): again the apparently untranslated 5' end is identical with that of R17. Moreover, because they succeeded in isolating a fragment of 129 nucleotides, they were able to go further and establish an overlap with the first cistron—that of the A-protein, which is identifiable by its sequence in R17. De Wachter *et al.* have thus verified that the A-protein cistron is indeed the first one in the chain, and that its initiation codon begins at position 130 in the sequence. The conservation of the 5' terminal sequence presumably reflects some highly specific function, the more so because the (again apparently untranslated) 5' end of the RNA of the quite unrelated Q β phage shows remarkable similarities to that of the R17 group.

Nichols and Robertson (*Biochim. Biophys. Acta*, **228**, 676; 1971) have meanwhile prepared other fragments of f2 RNA, which form part of the coat protein cistron. In 100 nucleotides there are four differences from the same tract in R17, one accounting for the single amino-acid difference between the coat proteins, another following after the termination signals for the coat protein message. The remaining two are inconsequential substitutions involving degenerate triplets, which do not change the message.

A different line in sequencing concerns the structure of the small ribosomal species, 5S RNA. This is of interest for a number of reasons, one of which is that as a small homogeneous species (but for one base) it provides a tempting objective for conformational divination. Until recently two 5S RNA sequences, those of

Escherichia coli and of human KB cells, were known. Now Jordan, Forget and Monier (*J. Mol. Biol.*, **55**, 407; 1971) have examined the 5S species from the inactivated ribosomes of chloramphenicol-treated *E. coli* and report that it differs only at the 5' terminus, where three different sequences are found, which are one, two and three residues longer than that of normal 5S RNA. These are presumed to represent precursors, such as are also found for the 16S and 23S components.

DuBuy and Weissman (*J. Biol. Chem.*, **246**, 747; 1971) have sought to resolve controversy about the base-pairing scheme of 5S RNA by determining sequences of other species. Those of three bacteria proved to be almost identical to *E. coli*, and they therefore selected for study a fourth, that of *Pseudomonas fluorescens*, a more distant species. The result was a sequence showing two-thirds homology with *E. coli*. The difference is sufficient to annihilate most of the numerous suggested pairing schemes for the latter, assuming that different 5S species are structurally similar. The long complementary sequences at the 3' and 5' ends remain, and also some other features. The situation is of

course complicated by the existence of two, or probably more, stable conformations, only one of which is functionally "native", in that it can be reincorporated into 50S ribosomes. An interesting approach to the conformation of 5S RNA is that of Jordan (*J. Mol. Biol.*, **55**, 423; 1971) who has studied the nature of nuclease attack on the molecule in its two characterized states. One takes it that the nuclease attacks preferentially in unpaired regions, and Jordan's results show that the pattern is quite different in the two forms of the molecule. Moreover, in the presence of urea, which weakens the base pairing, some of his fragments fall apart, by reason of breaks in the chain. By identifying the parts, as well as those fragments which have no internal breaks, Jordan has been able to go some considerable way to identifying paired and unpaired parts of the chain—not, however, to the point of defining the pairing scheme for the whole molecule; this can probably in fact not be represented at all in two dimensions, by contrast with the tRNA cloverleaf. Jordan's wholesome advice to the regiment of model builders at this stage is that they should be able to find more profitable ways to occupy themselves.

Reovirus Messenger RNAs

To be caught uttering clichés about the mysterious ways of nature may be embarrassing, but faced with having to interpret observations of the sort reported by Banerjee, Ward and Shatkin in next Wednesday's *Nature New Biology*, the imagination wilts. These workers have been investigating the transcription of messenger RNAs by preparations of reovirus particles and have come up with the curious finding that although these messenger molecules are synthesized with the starting sequence pppGpUp... one of the phosphate residues is cleaved from the first nucleotide to yield RNA chains which all begin with ppGpUp...

Reovirus, so called because, apart from being associated with respiratory and enteric infections, it is an orphan virus, which has not been shown to cause any specific disease, occupies a curious place in viral taxonomy. It is one of the few viruses which has a genome made of double stranded RNA. Each virus particle contains ten double stranded RNA molecules, which fall into three size classes, and each acts as a template for the transcription of ten corresponding single stranded messenger RNA molecules. The enzyme molecules responsible for this transcription, RNA dependent RNA polymerases, occur in the virus particles associated with the double stranded RNA genome, and this

of course makes reovirus particles an ideal system for studying transcription.

Banerjee and his colleagues knew from earlier studies that the first residue in at least some reovirus messenger RNA molecules was a guanosine but they did not know whether this was universally the first base or whether it was a diphosphate (ppG) or a triphosphate (pppG). To decide these issues they added to their incubation medium guanosine triphosphate (GTP) with ^{32}P in the second and third, or just the third phosphate groups. Completed reovirus messenger RNA molecules were only labelled with ^{32}P when the former was used. This result suggested that these RNAs contain a diphosphate as their first residue (ppG) and analysis of the products of the complete hydrolysis of complete reovirus messengers confirmed this; all ten species begin with the sequence ppGpUp except one which may begin ppGpCp. But GTP is incorporated as the first base of growing messenger RNA chains; in other words, they begin with the sequence pppGpUp...

Banerjee *et al.* therefore analysed the reovirus particles for an enzyme capable of cleaving one phosphate group from the first base of nascent messengers and found just such an enzyme, a GTPase which is an integral part of the virus particle from which it cannot be freed.

OCEAN FLOOR

Why Uniform Sediments?

from our Geomagnetism Correspondent

THERE is no doubt that the principal source of the sediment in the Atlantic is the continents—so much may be thought to be obvious, although there is good scientific backing for it as well. But how are such sediments transported to their resting places? Biscaye (*Bull. Geol. Soc. Amer.*, **76**, 803; 1965), for example, deduced that the topography of the mid-Atlantic Ridge controlled the distribution of clay minerals and thus concluded that they were transported there by bottom currents. Windom (*ibid.*, **80**, 761; 1969), on the other hand, came up with the less orthodox suggestion that anything from 25 per cent to 75 per cent of the sediment in the central Atlantic came there by way of atmospheric fallout. Either way, changes in the clay mineralogy with geological time might be expected because of likely changes in the winds or ocean bottom currents and even the sources.

But the surprising result of an examination of six cores from the Atlantic (19° N–23° N, 34° W–57° W) carried out by Murray (*Earth Planet. Sci. Lett.*, **10**, 39; 1971) is that, whatever the climatic or other changes over the past 200,000 years, they have had no significant effect on the clay mineralogy of Atlantic sediments. To be more specific, clay mineral abundances in the upper 2 m of the six cores were uniform within the limits of experimental error (10 per cent); and calculations of sedimentation rates showed the 2 m lengths to cover time spans of from 64,000 to 200,000 years. In other words, over a 20° longitude traverse of the Atlantic which includes the mid-Atlantic Ridge there is no evidence of clay mineral variations in spite of the fact that the minerals were partly deposited during the fluctuations in climate and sea level in the Pleistocene. Nor is there any evidence to suggest that the mid-Atlantic Ridge acts as a sediment barrier. At present, all this remains a surprise with no apparent explanation.

One of the six cores, however, was from the median valley of the mid-Atlantic Ridge and, though still uniform within the limits of experimental error, the clay mineral distribution in this core fluctuated more widely than in the other five samples. There were also other differences. There was, for example, an alternation of amorphous and crystalline material; and the amorphous layers were associated with silt-sized volcanic glass. Murray's view is that the amorphous layers are "the weathered products of volcanic glass layers whose original composition was similar to average ridge basalts". In other words, these glassy amorphous

layers seem to be related in some way to the intermittent volcanic activity in the area rather than to any continental source.

In fact, this is not the only evidence that some oceanic sediments are derived locally rather than from the erosion of continental material. For example, when Siever and Kastner (*J. Mar. Res.*, **25**, 263; 1967) discovered poorly crystalline montmorillonite in the median valley of the mid-Atlantic Ridge at 23° N, they concluded that they had found the low-temperature oxidation product of volcanic ash recently deposited. Then again, Bostram and Peterson (*Mar. Geol.*, **7**, 427; 1969) interpreted the chemistry of the East Pacific Rise sediments partly as precipitates from volcanic material derived from the rise crest. And Sayles (quoted by Murray) has found large amounts of poorly crystalline iron and manganese-iron rich sediments near the East Pacific Rise which seem to be of local origin.

FISH LARVAE

Giant Leptocephalus

by our Marine Vertebrate Correspondent

EEL larvae are strikingly dissimilar to the adults of the same species, for they are laterally flattened, shaped like an elongate willow leaf, and quite transparent. It is now a matter of history that at one time the larvae were recognized as a distinct group of fishes and given the generic name *Leptocephalus*,

although this name has lingered on as a general label for this type of elongate fish larva. A catch-all name of this kind has been very useful because few of the known larvae are certainly referable to their adult forms, and it is now known that a number of other fish orders have leptocephalus larvae.

In January 1930, the Danish Dana Expedition captured a leptocephalus on the Agulhas Bank, south of Africa, which was 184 cm long. This specimen has earned a place in the mythology of marine biology, for although Å. V. Tåning, who first reported its capture, merely claimed it to be the largest larval form within the animal kingdom, later authors seized on its length and, comparing it with the 90 mm maximum of the common eel (*Anguilla anguilla*) larva, produced a figure of 30 m for the estimated length of the adult!

Recent re-examination of this Dana specimen by Jørgen G. Neilsen and Verner Larsen (*Vidensk. Medd. Dansk Naturh. Foren.*, **133**, 149; 1970) shows that this larval putative giant sea serpent is morphologically very similar to a form described in 1959 as *Leptocephalus giganteus* from off New Zealand. The New Zealand specimen was relatively modest in size (893 mm), but careful comparison shows it to be otherwise identical with the larger specimen (although this is now in poor condition and measures only 1,330 m, having dried out while on loan from Copenhagen). *Leptocephalus giganteus* have also been found offshore of Florida, and D. G. Smith (*Copeia*, **1**;

Further Evidence for Fe³⁺ in Interstellar Dust

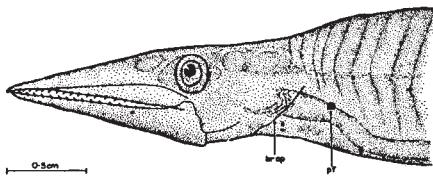
P. G. MANNING has come up with some more support for the view that minerals containing the Fe³⁺ ion make up a significant fraction of the interstellar solid material. Writing in next Monday's *Nature Physical Science*, Manning describes an analysis of the spectrum of the supernova which appeared in NGC 3003 in 1961, and attributes some of the features in the spectrum to transitions in Fe³⁺ ions. The chances that iron is one of the many components of interstellar dust must be high—iron will be manufactured by many stars and released into space—but whether the features indicated by Manning in the supernova spectrum are indeed attributable to Fe³⁺ could, however, still be an area for considerable debate.

Partly, this will be because of uncertainties about the genuineness of the bands which Manning is discussing, although, to be fair, their existence is virtually certain. More likely, people will ask whether the bands can be

accounted for in any other way, because one of the hazards of this kind of work is that it is notoriously simple to manipulate the proportions of the ingredients of interstellar dust to simulate a variety of spectra.

In next week's report, Manning attributes three bands in the ultraviolet part of the supernova spectrum and one in the visible part to Fe³⁺ ions in octahedral sites. The spectrum of the supernova in NGC 3003 is odd in that it is the only spectrum in the Type IV classification suggested by Zwicky, but from Manning's work it seems just as feasible to explain the absorption bands in this spectrum by Fe³⁺ ions as it has already proved to explain the spectrum of Type I spectra in this way. What the Fe³⁺-bearing minerals are is not known, but Manning points out that garnets are still a possibility. Work on laboratory spectra of minerals in simulated space environments might help to throw some light on this aspect of the problem.

1970) has recently demonstrated that this form is the larva of a notacanthiform fish.



Head of *Leptocephalus giganteus*.
br ap, Gill opening; pf, pectoral fin
(from *Vidensk. Medd. Dansk Naturh.*
Foren., 133, 152; 1970).

Nielsen and Larsen concur with this view, and suggest that all the giant leptocephalus larvae belong, not to the order of eels, but to the notacanthiform fishes. They also describe four other large Dana leptocephalus larvae which range in length from 83 to 450 mm, and were captured off the west coast of North Africa in March 1930. These show differences from the *L. giganteus* form, and Nielsen and Larsen suggest that they are probably referable to the same genus or generic group of notacanthiform fish.

The notacanthiforms are bathypelagic and benthic fishes of the deep seas. They include the halosaurs, which are known to develop from leptocephalus larvae, and the spiny eels or notacanth. In general, little is known of their biology and it is not yet possible to suggest which species develops from the giant leptocephalus, if indeed the adult form is yet known to science.

BINARY STARS

Measurements of α Vir

from a Correspondent

ASTRONOMERS at the University of Sydney have determined, for the first time, all the important physical parameters of the binary star system α Vir. They have observed α Vir with the stellar intensity interferometer at Narrabri, and their results clearly demonstrate the great potential value of this type of instrument in the study of close binary systems (R. Hanbury Brown *et al.*, *Mon. Not. Roy. Astron. Soc.*, 151, 161; 1971). Hanbury Brown and his colleagues studied α Vir on twenty-eight nights in 1966 and 1970, and because the orbital period is only four days they were able to follow the star through several complete cycles and thus obtain the variation of correlation with different phases of the orbit.

Spectroscopic and photometric observations of α Vir are adequate to calculate with precision four of the orbital parameters, and these well established values can be used as a

check on the interferometric data. The inclination of the orbit to the line of sight cannot be found spectroscopically, however, and this precludes calculations of the distance to the system and masses of its components.

The interferometer gives a direct measure of the inclination, and so by combining interferometric and other optical data the Sydney workers are able to give a complete description of α Vir. Among the most important new results is the star's distance, which is 84 ± 4 pc (1 parsec = 3.26 light years); this compares with three earlier trigonometrical determinations giving 111, 59, and 34 pc. The new distance estimate relies on measurements of velocity and angle only, and is totally independent of uncertain quantities such as interstellar reddening or estimated luminosity.

Other valuable data on the stars

include the absolute luminosity and magnitude, the temperature and the radius of each component; the primary and secondary components are 10.9 and 6.8 solar masses respectively. The new results represent a further empirical point on the mass-luminosity and mass-radius curves which have been scantily determined for the early type stars. α Vir is consistent with the information derived from other binary stars, although it has probably evolved away from the main sequence.

This preliminary experiment on the application of the Narrabri facility to a complex problem shows that an instrument of greater sensitivity could establish the distances of binaries which are beyond the reach of classical trigonometry. Such a development would be warmly welcomed as an extension to the present meagre knowledge of the physical properties of hot stars.

Transplantation Antigens in Rhesus Monkeys

A PARTICULARLY interesting stage has been reached in the study of so-called strong transplantation antigen systems, the antigenic systems which have the most powerful influence on the survival of incompatible grafts. All species so far studied seem to have a single strong system and a series of weaker ones. From work reported by H. Balner *et al.* in next Wednesday's *Nature New Biology*, the same pattern seems discernible in the rhesus monkey.

Balner and his colleagues have identified twelve antigenic specificities on the leucocytes of rhesus monkeys, and in an analysis of two hundred unrelated monkeys they found that the specificities could be arranged into two sets. Within each set the distribution of antigenic specificities in the two hundred unrelated monkeys showed positive associations, whereas between sets negative associations were frequently seen. The most likely explanation of these associations is that the leucocyte antigens detected belong to a single, probably complex system analogous to the human HL-A and mouse H-2 systems. The analogy with HL-A was made more convincing by the demonstration that some monkey families could be genotyped and the antigens were found to segregate in a pattern consistent with a single complex system. Furthermore, skin grafts exchanged between genotypically identical siblings survived significantly longer than grafts made between non-identical siblings.

The RhL-A system, as Balner's group suggest it is called, has potential importance for a number of reasons. One is that before bone marrow transplantation can become a safe and useful clinical procedure many factors still

require study, preferably in primates. Analysis of the RhL-A system will make it possible to carry out these studies in monkeys where the antigenic gap between donor and recipient has been clearly defined. Another reason for interest in RhL-A is that it may help in resolving questions concerning the genetic structure of strong transplantation antigen systems. Until recently, the mouse H-2 system was believed to consist of a series of closely linked genes determining up to fourteen pseudoallelic antigens in any one inbred strain (Graff, R. J., *Transplant. Proc.*, 2, 15; 1970). This concept is quite different to that of the HL-A system which is currently thought to be composed of two closely linked genes determining two series of allelic antigens (Kissmeyer-Nielsen, F., and Thorsby, E., *Transplant. Rev.*, 4, 1970). Because H-2 and HL-A substances seem physicochemically to resemble each other closely, it seems unlikely that the two systems would have greatly different genetic structures, nor does this seem plausible from an evolutionary point of view. Recent evidence (Snell, G. D., Cherry, M., and Dement, P., *Transplant. Proc.*, in the press) shows that some (but not all) H-2 antigens can be placed into two segregant series, and that the definition of other H-2 antigens may have to be questioned (Thorsby, E., *Europ. J. Immunol.*, in the press). The previously held concept of H-2 genetic structure is now very much open to doubt. If further work shows that the RhL-A system also consists of two segregant series of antigens, it is bound to undermine even more the concept of H-2 as a system of multiple, pseudoallelic antigens.

TISSUE CULTURE

Factors for Growth

from our Cell Biology Correspondent

SERUM is the cocktail of life to most mammalian cells grown in tissue culture; without it they are unhappy, they cease to multiply and if deprived for more than two or three days they die. This requirement for serum is of course a great handicap, for it means that most cells cannot be grown in a defined and precisely reproducible medium; batches of serum vary and their composition is unknown. But there are two sides to every coin. If the factors in serum essential for survival and for the initiation of DNA synthesis and the cell cycle could be isolated pure, investigation of the way they act might throw considerable light on the questions posed by the existence of processes which regulate cell multiplication.

As the slow progress of the group at the Salk Institute centred round no lesser a biochemist than Holley testifies, the task of fractionating growth and survival factors from sera is proving to be far easier said than done. But after several years work the Salk group has now reached the point, as Paul, Lipton and Klinger report in the current issue of the *Proceedings of the US National Academy of Sciences* (68, 645; 1971), where they can isolate, partially pure, no less than three factors which control the initiation of DNA synthesis in 3T3 cells or 3T3 cells transformed by SV40 (SV3T3) and a fourth factor necessary for the survival of these cells in culture.

Apart from the straightforward technical problems encountered when a complex mixture of molecules is fractionated, isolating growth and survival factors hinges on effective assays for these two properties. Paul *et al.* assayed growth factor activity by measuring ³H-thymidine uptake by cells maintained in a medium containing 0.4 or 0.2 per cent serum, enough to ensure survival of the cultures but no significant increase in the cell density. They measured survival factor activity by plating cells in a medium lacking any serum to which is added the fraction being assayed. A cell count 3–4 days later revealed if the fraction caused the cells to survive.

The growth promoting activity of whole rat serum proved to be stable over a wide pH range and, to cut a long story short, Paul *et al.* eventually developed a fractionation technique to exploit this fact. After a preliminary electrophoretic separation at pH 8.6 on cellulose acetate, which partially separates a broad peak of activity which promotes DNA synthesis in 3T3 cells from a similar activity for SV3T3 cells, gel filtration on 'Sephadex G100' at pH 2.0 resolves three distinct peaks of activity (peaks I, II, III). All three con-

tain non-dialysable trypsin sensitive material which is presumably protein. Peaks I and III stimulate DNA synthesis by SV3T3 cells, 3T3 cells transformed by both polyoma virus and SV40 and the so-called flat revertants of SV3T3 cells isolated by Pollack (cells which grow like untransformed cells but retain the SV40 genome). These two factors, which in combination have an additive effect, seem therefore to be specific for cells carrying a tumour virus genome. By contrast the factor in peak II only stimulates DNA synthesis by untransformed 3T3 cells. But in spite of their ability to stimulate DNA synthesis, none of these factors can keep alive cells plated in media lacking all traces of complete serum. A fourth and distinct factor in complete serum, which on electrophoresis runs with the alpha globulins but is apparently distinct from them, seems to be essential for the survival of cells in culture.

Now that Paul and his colleagues have broken the back of the serum fractionation problem, more rapid progress towards the goal of complete purification and characterization of these four factors can now perhaps be anticipated. Furthermore, following Holley and Kiernan, Paul *et al.* have found in

human urine factors similar to those in sera which seem to promote DNA synthesis.

This observation is of course important because it is far easier to collect urine than, for example, rat serum and it may therefore be possible to isolate larger quantities of these factors from this novel source. Then the problem will be discovering how the factors act. And it should be possible to test Balk's suggestion (*Proc. US Nat. Acad. Sci.*, 68, 271; 1971) that the mitogenic factors in serum are released from precursors in plasma or from thrombocytes when blood is clotted during the preparation of serum.

GREAT LAKES

Field Year Programmes

from our Geomagnetism Correspondent

THE Great Lakes are a man-made disaster area by any standards and as a result a great deal of effort is being devoted to fundamental research in the area simply to find out what basic processes operate, how they are being affected by man and thus how the resulting eutrophication can be slowed

New Light on Regulation of Plant Senescence

ALTHOUGH the effects of light on the growth and early development of plants are well known, the influence of light on senescence in plants has been little studied. In the course of experiments designed to test the effect of treatment with low intensity white light on the grain yield in wheat, P. R. Walpole and D. G. Morgan of the University of Cambridge have uncovered a novel effect. It seems that if young wheat seedlings are exposed to an extended period of low intensity light, the senescence of the principal photosynthetic tissue is markedly accelerated.

Walpole and Morgan vernalized—in other words, promoted germination by chilling—wheat seeds for four weeks before growing the plants to the third leaf stage in a cool greenhouse. The plants were then transferred to a controlled environment chamber in which the daylength was set at 1 h and the light intensity was 2,850 foot candles at pot height. The plants were then transferred in groups of ten at weekly intervals to a second controlled environment cabinet in which the light intensity was only 700 foot candles at pot height. The groups were left in the low light conditions for one week before transfer back to the high light intensity environment.

This light regime delayed anthesis, that is the emergence of the anthers in

the flower head, for two to three days, but only the light stress imposed when the plants were at an early stage in the development of the flower head markedly depressed the yield of grain from the ear. This reduction was due not to a reduction in the number of grains per ear, but to a decrease in the mean grain weight.

It seems likely therefore that this decrease in yield could be the consequence of some effect of the earlier low intensity light on the photosynthetic efficiency of the flag leaf area, the large area of green tissue above the flag leaf node which seems to take the major role in determining cereal yield. Walpole and Morgan, in fact, found that the low intensity light treatment accelerated yellowing of the photosynthetic tissue in this region, the loss of chlorophyll resulting in decreased photosynthetic efficiency.

Walpole and Morgan suggest that the low intensity light treatment may cause some subtle change in the balance of plant growth hormones which persists to modify the normal pattern of senescence. Equally these results could be interpreted as a discreet change in the way in which senescence may be coded in the genetic material. Whatever the mechanism, these studies have clearly opened a new approach to the problems of plant senescence.

down to something approaching natural rates.

During the International Field Year for the Great Lakes (IFYGL), a period of intensive data collection scheduled for 1972, an assault will be made on Lake Ontario and its basin which will provide a comprehensive synoptic description of the physical process at work there and, by implication, in all large lakes. It was originally intended that the studies should be grouped into four core programmes—lake meteorology, energy balance, terrestrial water balance and water movement—though a comprehensive biological and chemical study of the Lake Ontario waters has since been added.

Water levels on the Great Lakes vary by up to two metres or more as a result of the weather, changes which affect water supplies, navigation and shoreline activities. Conversely, the lakes themselves affect the weather in their vicinity. So the lake meteorology programme of the IFYGL will involve high altitude aircraft, balloon and satellite measurements of moisture content, and wind and cloud patterns in the Ontario basin. The use of Lake Ontario as a "model ocean" for the study of exchange processes for heat, moisture and momentum between the water and the atmosphere will also contribute to the Global Atmospheric Research Program (GARP).

The terrestrial water balance programme is nothing more or less than a search for and an assessment of the water sources and sinks in Lake Ontario. Clearly a thorough knowledge of the geology of the basin, and especially the water-bearing characteristics of the rocks, is required here; and a base map of the Lake Ontario basin has already been completed. Particular attention will also be paid to the role of aquifers in maintaining the water balance in the basin, for an understanding of the interaction between aquifers and the lake will improve the usefulness of these aquifers as reservoirs. The water movement programme, on the other hand, is concerned with diffusion and circulation within the lake itself.

The energy balance programme is perhaps the most fundamental and far-reaching of all, for the thermal structure of the lakes directly affects wildlife, fishing, water supply, shipping and agriculture. Measurements made during the IFYGL will therefore include incoming and outgoing long and short-wave radiation, latent and sensible heat transfer, heat storage and the formation and dissipation of ice. But all this concerns physical properties. The fifth core programme will focus on the interrelationships between the distribution of biota in the lake and the chemical characteristics of the water.

VISION

Binocular Connexions

from our Neurophysiology Correspondent
FROG tadpoles have no binocular vision: their eyes point sideways so that the two visual fields cannot overlap. During metamorphosis the eyes rotate to point forwards, allowing a small amount of overlap so that there is a restricted part of the central visual field within which binocular vision is possible. For monocular vision each retina need only project to its own half of the central receiving area for vision, the contralateral optic tectum. For binocular vision, however, input from both retinas is correlated, so that the binocular projections from each retina must meet. This is made possible by the development of an ipsilateral projection for each eye. For example, the left eye normally projects to the right tectum; after metamorphosis there is an additional projection from neurones in the

right tectum, representing the binocular area in the left retina, to the corresponding neurones in the left tectum.

"Binocular" neurones in each tectum thus receive information from conjugate points in both retinas. This allows the measurements of retinal disparities and the localization of objects in the depth plane: the frog can catch flies.

The ipsilateral projections could be formed as a result of neural growth using only genetic information—as are the original contralateral projections—or they could be formed by the directed growth of fibres to link neurones in both tecta that were simultaneously active. This second alternative would mean that neurones responding to the stimulation of conjugate retinal points would automatically be connected. It also implies that ipsilateral projections could develop only after metamorphosis and in a lighted environment. One drawback, of course, is that it would be difficult to distinguish contributions

Multiple Copies of Histone Genes

IN next Wednesday's *Nature New Biology*, Kedes and Birnstiel present convincing evidence that the genome of the sea urchin *Psammechinus miliaris* contains many copies of the genes which specify histones. Furthermore their work suggests that it may be possible to isolate these histone genes from the remainder of the DNA.

The notion that the genomes of higher plants and animals contain reiterated genes and other DNA sequences is by now universally accepted. For not only do the cells of higher organisms contain too much DNA for it all to have unique sequence but also a variable but large proportion of the DNA of higher organisms renatures very rapidly after denaturation. That means, of course, that many of the sequences in the DNA can readily find their complements, which in turn implies that there are many repeated copies of those sequences.

But which sequences are highly reiterated and are any or all of them used to specify functional proteins or RNAs? It is known that in some species of rodents, and probably in most organisms, part of the rapidly renaturing DNA contains multiple copies of very short sequences which do not code for any protein or RNA but may well have some structural function. It is also known that in many organisms multiple copies of the ribosomal RNA genes occur clustered together on DNA molecules. Now Kedes and Birnstiel have added the histone protein genes to this list of reiterated sequences.

They have isolated partially purified preparations of three species of histone

messenger RNAs from cleaving sea urchin embryos and used them as probes for the corresponding genes. They found that the histone messenger RNAs hybridize rapidly with an excess of denatured sea urchin DNA; from the kinetics of this hybridization and the stability of the hybrids they estimate that there may be as many as four hundred copies of the histone genes in each genome.

There is also some evidence to suggest that these repeated genes are clustered together on DNA molecules and if that is the case their isolation becomes a feasible proposition. Kedes and Birnstiel estimated the overall base composition of histone genes from the known amino-acid sequence of one species of histone. Their estimate is inevitably very approximate, but it suggests that histone genes are rich in the bases guanine and cytosine; they are therefore likely to accumulate in the denser fractions of a sheared sea urchin DNA preparation. And, sure enough, histone messenger RNA hybridizes with fractions of sea urchin DNA, the density of which is greater than the average. With luck it should be possible to separate these comparatively dense DNA fragments which carry clusters of the sequences complementary to histone messenger RNAs by much the same techniques that Birnstiel and others have used to isolate the clustered and reiterated ribosomal RNA genes. And once that has been achieved, a host of fascinating questions related to gene transcription, the evolutionary stability of the histones and the like might at last be answerable.

from stimuli in different depth planes, as they would have conjugate points at different retinal disparities.

In any event, Gaze, Keating, Székely and Beazley (*Proc. Roy. Soc. B*, **175**, 107; 1970) performed experiments the results of which seemed to favour the "post-metamorphosis" hypothesis. They found that the contralateral projection from an eye that had been rotated at an early stage in development was also rotated, but that the ipsilateral projection went to the correct part of the ipsilateral tectum. Furthermore, the ipsilateral projection from the untouched eye was rotated, whereas the contralateral projection was normal. From these results Gaze *et al.* argued that the ipsilateral fibres could only have grown, under conditions of light stimulation, between neurones that were simultaneously activated by input from each retina. Their experiments were on *Xenopus*, the clawed toad, once a central figure in pregnancy tests. Jacobson has now repeated these experiments, using a frog, and has added some of the missing controls (*Proc. US Nat. Acad. Sci.*, **68**, 528; 1971).

Jacobson found that animals kept in the dark from larval stage XVII, through metamorphosis, developed normal ipsilateral projections. Indeed, he could detect, by electrophysiological mapping techniques, ipsilateral responses as early as larvae stage XIX, in normal larvae. These results are not consistent with Gaze *et al.*'s theory, nor is the further result that occlusion of one eye with a skin graft from an early larval stage does not prevent the development of a normal ipsilateral projection. If the skin graft is retained for more than about 120 days, however, the ipsilateral projection becomes disorganized, implying that simultaneous excitation by way of the stimulation of corresponding points on the two retinas may be necessary for the maintenance of binocular function. Jacobson also found that the projections from rotated eyes were rotated, so that there was always an angular disparity between the ipsilateral projection of a rotated eye and the contralateral projection of its unoperated neighbours.

Assuming that Jacobson's and Gaze *et al.*'s results are comparable in spite of the difference in species used, it seems that there is clear evidence against the view that binocular connexions are determined by visual stimulation, or "experience". They must therefore be genetically determined. The mapping techniques used by both sets of workers are quite crude, however, so it is possible that coarse binocular connexions are made during metamorphosis, but that these become more precise as a result of visual experience. Certainly, binocular excitation is required for the maintenance of these connexions.

Radio Waves guided by the Earth's Surface

ELECTROMAGNETIC waves are often guided in the laboratory by electromagnetic plumbing in the form of waveguides but it would seem that it is also possible to use the Earth's surface in a similar manner. In next Monday's *Nature Physical Science*, an extensive study of the propagation of Earth surface waves, including the design of launchers for such waves, is described by A. F. Wickersham of the Stanford Research Institute, California. He points out the possible commercial advantages of radio transmission by surface waves (absence of ionospheric fading and small interference between neighbouring stations, for example) and predicts, perhaps a little over-enthusiastically, that a range of 3,700 miles over seawater at 2 MHz might be a possibility.

This is not the first study of Earth surface waves to be undertaken but all the others have relied on the generation of a small amount of energy in the form of transverse magnetic waves by conventional antennas most of whose output is in the transverse electric and magnetic (TEM) mode. Wickersham has used two sizes of tubular launcher to investigate TM surface waves at 115 and 252 MHz. He has also employed long aluminium sheets about 30 cm wide in a successful effort to enhance the quantity of energy received at a distance from the launchers by detectors of flat or tubular design.

One of the principal methods of

examining surface wave properties is to measure the falloff of received power with distance from the launcher. For a particular form of TM surface wave known as the Zenneck wave an exponential behaviour would be expected, but an inverse cubic dependence on distance is also possible for other types. The results at 115 MHz over a clay field seem to indicate exponential behaviour but at 252 MHz the cubic law seems to fit the experimental data better. At 252 MHz over salt water and wet clay there is again evidence for a cubic dependence, and because the measurements at this frequency extend to ranges of about 10^4 m (at which distance the curvature of the Earth is significant) this is ample evidence of surface wave propagation. If all the data are assessed together, the picture that emerges is of exponential propagation at short distances, cubic propagation at large distances and anomalous propagation at intermediate distances (such as an increasing received power with increasing range).

Wickersham also carried out some experiments with more conventional antennas at 6.91 MHz and found that the radiation component at the surface resembled a surface wave but obeyed a $1/R^4$ law which is in close agreement with a flat Earth rather than a curved Earth theory. No doubt future work on the propagation mechanisms will satisfactorily explain this strange finding.

The Age of Ethiopian Flood Basalts

In spite of the importance of the region, so little is known about the geology and geophysics of the African Rift system that any new data are very welcome indeed. But although it is nice to see an oasis after extensive travel in the desert it is even better to come upon a lush river valley. The two potassium-argon dates reported by Rex *et al.* in next Monday's *Nature Physical Science* are thus important; but it would have been even more useful to see ten times that number. In the early days of potassium-argon dating, when wonder at the technique was still rife, it was common to see papers reporting but one or two dates. But in this age of the mega-collection for palaeomagnetic and other purposes, it is a little surprising that so few dates are considered sufficient unless, of course, the rest of the collection failed to meet the stringent criteria necessary to ensure success in the use of the particular technique.

This is not, however, to detract from

the quality of the two ages that Rex *et al.* report, nor to underemphasize their intrinsic importance. The dates came from two ignimbrite layers from among the vast flood basalts associated with the Ethiopian Rift and in a limited way put the whole system into some sort of age perspective especially in relation to the even more limited data previously available. The Trap Series of central Ethiopia, for example, is often regarded as Eocene-Oligocene, a range which receives some support from a 49 ± 15 million year radiometric age from the Blue Nile gorge. A palaeomagnetic study of rocks in the Addis Ababa area, on the other hand, has yielded a pole position more consistent with the Miocene. This highlights well the problems inherent in too few data. It would, in turn, be dangerous to conclude too much from the new dates of 9 and 21 million years. What they do suggest, however, is a long period of basalt extrusion in the Ethiopian Rift system.

Science and Antiscience

ERIC ASHBY

Clare College, Cambridge

This article highlights some of the points made by the author in the inaugural Bernal Lecture* delivered at the Royal Society on March 4, 1971.

My theme starts from the paradox that the crisis of disillusionment in western affluent societies was the successful landing of a man on the Moon. Until then large numbers of people were still prepared to believe that the social benefits of science and technology were largely fortuitous and that the disorders of society were largely inevitable. But at that point people realized that a wealthy nation could mobilize enough skill and money deliberately to solve an incredibly difficult technological problem. At the moment of triumph there was criticism, not of the achievement but of the goal. Many Americans¹ regarded walking on the Moon as "an arrogant piece of conspicuous consumption". If this is what a sustained effort of planned technology can do, why is planning not successfully applied to other goals; why not to the transport problem in cities, or to poverty, or to the relief of the Third World? It is not convincing to say to the man in the street (though I believe it is true) that these problems are more difficult to solve than that of putting a man on the Moon. People feel powerless to influence the goals to which the national effort in science and technology is directed. Some of them now resent this and their natural response is either to press for strict public control guided by ethical imperatives, or to withdraw financial support—to regulate what scientists do or to starve them. So we are faced with a problem which is easy to state but hard to analyse: Is it part of the social function of science to determine goals?

But it is not only the US Congress which is questioning the scale of investment in science. There is in the United States, and to a less extent in Britain, a widespread unease (amounting among some of the young to despair) because moral skills applied to social institutions have not kept pace with scientific skills applied to technological needs. Society, regarded as a non-linear feedback system, is showing the signs of oscillation ("hunting") which one expects when the homeostatic mechanisms, which should maintain an equilibrium between political decision making and the state of technology, are responding too slowly. Examples in Britain are the vacillations in our policies for dealing with higher education, urban transport, and the aircraft industry. These surges of uncertainty are exacerbated by the instant visibility of crises, brought into our homes by television. We become hypersensitive to the contrasts between poverty and plenty, violence and the enforcement of law and order, the occasions of failure to choose socially desirable goals and the successful technology which achieves the goals which are chosen. There is an irony about all this. Over a generation ago the advanced western countries seriously upset the equilibrium of traditional communities in Africa by exporting technology to them; now the disequilibrium has spread to our own cities and affected our own communities.

One has only to look east to realize that political systems

* The Lecture will be published in full in the B series of *Proceedings of the Royal Society*.

different from our own are not much more successful in adapting society to its contemporary environment. It is significant that zealots of the New Left reject the communist pattern of politics as vehemently as they reject the capitalist pattern. They have no coherent alternative but—and this is important—many of them have cultivated an ideology of antiscience. They question the very legitimacy of scientific thought. This is nothing new; indeed, Bernal recalls² how Georges Sorel, over seventy years ago, repudiated the intellectuals of his time in favour of an emphasis on instinct and intuition; and Bernal warned his readers that this is the stuff of which fascism is made.

Bernal's warning should not be ignored, nor should this recrudescence of antiscience in the 1970s be written off as the aberration of a few hippies. It has little in common with the antiscience of the nineteenth century which pestered Darwin and Huxley. Its supporters are not drawn from the clergy or from simple people unfamiliar with science. Indeed, some of its propagandists are scientists themselves, who complain that science—once frustrated by the rigours of poverty—is now frustrated by the demoralization of wealth. These people do not seriously suppose that the efficacy of scientific thinking for the understanding of natural phenomena is passing; we are not at a watershed similar to the one which divided Aristotle and the schoolmen from Galileo and Newton. But we are at a point of time when it is being seriously suggested that science ought to be practised under some sort of public scrutiny and restraint. In a recent book on science and society³ the authors ask how control over scientific affairs can be asserted; they go on to suggest that the committees which "allocate resources between disciplines and fields in the basic sciences must do so in the context of politically directed goals set by the community", and that the present "oligarchies" (presumably the research councils) should be replaced by persons "openly elected by the scientific community from amongst its own number". The threat to society is⁴ "the paternalism of expertise within a socio-economic system which is so organized that it is inextricably beholden to expertise". The innuendo is that if experts could be made irrelevant there would be no need to go through the tiresome process of becoming one. The counter culture offers a path to wisdom about nature which does not lie along a long dusty road of hard work. The more sophisticated prophets of antiscience, such as Ellul and Marcuse, are more reactionary than this. They encourage disaffiliation from the contemporary scientific culture on the ground that if its values are adopted, mankind will become enslaved by his technocracy. Indeed, they say, it is already happening. They should also not be dismissed as querulous eccentrics. For public enthusiasm for science, and confidence in scientists, and undisputed willingness of nations to finance research, are very recent phenomena; it is only about twenty-five years since they appeared and they could just as quickly disappear. So we should not leave unchallenged those who wish to impose politically directed goals on basic (or, as Americans call it, "discipline oriented") research, and those who embrace the counter culture of antiscience. They confuse, in my view, two quite different issues—the purposes for which science is done by scientists and the purposes for which science is used by society. Although this distinction is a trite one, it is necessary to restate it for the sake of my theme.

Choice of Goals

The achievements in basic research are possible only because the research worker can disregard first causes and any purpose except that imposed by the inner logic of the discipline itself. He is therefore able to choose problems on the two criteria that they are likely to be soluble and that the solutions will be relevant to current concepts in the discipline. The use of these criteria has one unfortunate side effect, namely that the intractable problems come to be regarded as unimportant because they are never tackled. (Thus classical problems in biology, such as the origin of a land flora, are neglected not because they have been solved but because they seem at present to be insoluble.) But the framework of concepts in science owes its coherence and strength to the fact that those who build it do not try to comprehend reality; they build from abstractions and simplifications. So it is evident to anyone who has done basic research that the problems to be tackled cannot be defined by persons outside the discipline and that the solutions are valid only within the framework of the discipline.

The situation is different for mission oriented science because the goal lies outside the discipline. The actual scientific work is still done by the methodology of basic science, but its intrinsic purpose is mediated by an extrinsic purpose. The influence of the extrinsic purpose may be weak, as in some kinds of medical and agricultural research which have a long range generalized goal such as a cure for cancer or the production of a drought resistant wheat; or it may be strong, as in research to increase profits in industry or to devise defence against missiles. The old controversy, which has recently been sharpened by the Society for Social Responsibility in Science, is whether the scientist is competent to define the extrinsic purposes. Is it part of the social function of science to determine goals?

The choice of extrinsic goals cannot be determined by the methodology of science. There are some who believe that if there were big enough computers and clever enough programmers and reliable enough data, the social sciences could do for politics what the physical sciences have done for matter. I do not share this belief, for two reasons. One essential input for political decisions is the prediction of public opinion toward the decisions, and the very process of seeking public opinion frequently changes it. No complete description exists which would be equally valid whether or not the units were informed of it⁵. Another essential input for political decisions is value judgment and this cannot be satisfactorily quantified. The social sciences can quantify the costs and benefits of this or that political choice; what they cannot do is to quantify, even through statistically analysed questionnaires, the value of the choice. It is on grounds such as these (though of course the conclusion is arguable) that I conclude that no mathematical refinement of the social sciences will enable rigid scientific methodology to be applied to the choice of goals for mission-oriented science. To this extent the anti-scientists have a point. What they are reminding us—though they deplorably distort their own case—is that the scientific method can speak authoritatively about means in society but it cannot be authoritative about ends. There is no straight path from fact to value. If we rely on science alone, questions of purpose will not be answered; and politics are about purpose.

Position of Scientists

The policy of government, that scientists should be "on tap but not on top", which was at one time briskly challenged by scientists, is now commonly admitted to be a correct policy by those who have a right to an opinion (ref. 6, for instance). Attachment to the methodology of science is, if anything, a disqualification for decision making in politics; there was wisdom in the cynical remark of a Cambridge citizen that he would rather be governed by the first hundred persons whose

names appear in the telephone directory than by the faculties of the University. The formula for success in science—simplification and abstraction—can be disastrous in politics. Nevertheless the methodology of science does have a powerful contribution to make toward the choice of goals in mission oriented research. It can introduce considerations which otherwise would not be taken into account. One familiar example is research into pesticides. The original primary goal, in government sponsored research, was to control pests and, in industry sponsored research, to make profits. But in those early stages it was nobody's business to examine third party interests, and it was not until persistent organochlorines had spread along the food chain and accumulated a thousand-fold in the bodies of birds that the goals of research were changed. The purpose of applied research in this field now is to produce non-persistent pesticides. But how can these third party interests be safeguarded? A panel convened by the National Academy of Sciences has proposed that there should be an independent body to assess the consequences of new technologies and to make public the third party interests before the technology is introduced⁷. One should not expect more than limited benefits from such a body, for it is notoriously difficult to predict the side effects of new discoveries. Anything which smacks of research into the future is bound to generate scepticism and invite contradiction.

Making Choices

By the cautious use of modern statistical methods, very useful help can be given to those who have to make choices in areas of uncertainty, and scientists, through their expertise in science, can influence these choices. But when it comes to making the extrinsic choice itself, the scientist steps outside the logic of his discipline, and has to admit non-cognitive elements into the equation. Like everyone else, he is expected to work within the political system which sustains him, and to accept the fact that it is the politician, not the scientist, who is elected to represent the value judgments of the people. But there is another—and to my mind more effective—way in which scientists could influence the choice of goals.

The traditional functions of government are to maintain law and order, to defend the interests of citizens against foreigners and to preserve the ethos of the nation. Our traditional political system is adapted to these functions, but modern governments have an additional function which has brought a new order of complexity into public affairs, namely to supervise and guide controlled technological and social change. It is hard to ascertain the will of the people over this new function because the issues are believed to be too difficult for the people to understand or, for that matter, for their representatives to understand. Too often parliaments seem to be no more than passive observers of such change. This accounts for the naive pressure from the radical left to return to the participatory democracy of a New England village. Our system of higher education, which has been the formal apprenticeship for most of the technological goal setters in Britain, gives no training for politicians and administrators in how to use the inputs of science in the making of political decisions and none for scientists in how to give due weight to non-cognitive considerations in choosing extrinsic goals for science. When, for example, do the results of science justify political action? Should cigarettes be as illegal as pot? Should women in overcrowded countries be obliged to take the pill under risk of punishment. Should airlines pay, through airport charges, for the noise pollution they cause, and the proceeds be used to provide double glazing in all homes within ten miles of the runway? Neither the scientist nor the politician can gain an expertise for dealing with such questions from our system of higher education.

Science in Universities

On this argument rests the case for a reappraisal of the function of science in universities and other institutions of

higher education. But first there has to be an understanding of the forces which shape these institutions.

A university, like an organism, is a product of heredity and environment. Its heredity is manifest as a consensus among academics about what a university stands for: excellence, objectivity, the cultivation of reason, the inherent value of knowledge; all the clichés emitted by vice-chancellors at graduation ceremonies. This consensus, if strong enough, generates a powerful inner logic, which is inherited by new universities. The university's environment is the social and political system which supports it. This acts through two main forces; the pressure of candidates to get in (customer demand) and the suction from employers drawing graduates out (manpower needs). The flow of public finance into universities depends on whether the public are satisfied that due weight is being given to these two forces. So in all universities there is a dynamic equilibrium between three forces: inner logic, pressure and suction.

British universities are at present in a state of disequilibrium between these three forces. The Robbins Report, wisely as it turned out, declined to base its calculations on predictions of manpower demand. But, less wisely, it had almost nothing to say about what the goals of the system should be; it was assumed that the old inner logic would persist. All but a few of those now responsible for university policy are busying themselves with similar logistic problems: How should the predicted student population of 1980 be divided between universities and colleges in the public sector? Can the costs of mass higher education be trimmed by fuller use of buildings, a longer academic year, diluted staff-student ratios, student loans, residence at home? Those who speak for the universities have made their attitude clear. They dislike all the proposed measures to cut costs (even fuller use of buildings reduces flexibility and narrows the options open to students) and they defend the tenets of their conventional inner logic: no devaluation of the degree, no erosion of research, and no more than a tentative sacrifice of depth for breadth.

Some critics denounce the policy of expansion by asserting that "more means worse". They have yet to make a convincing case and such evidence as we have is inconsistent with their assertion. But already there is no doubt whatever that "more means different". If this consequence of expansion is not pursued, universities will find themselves giving higher education to about 14% of the age group in 1980 on the same assumptions as they held when they were giving it to about 4% of the age group in 1960. Students and the public are already questioning these assumptions.

The traditional function of universities is mission oriented and is to educate a selected cohort of people to serve society; research is a function added only recently. Service to society calls for skill in resolving problems arising from social, technical, and psychological conflicts, and the equally difficult skill of living with the problems which cannot be resolved.

The contrast goes deeper. Within disciplines the homeostatic mechanism, the adjustment of goals to circumstances within the discipline, is astonishingly efficient. Despite the volume of material published or circulated as preprints, the more enterprising workers in a discipline soon get to know of important work being done in other laboratories, and in their own research they respond promptly to this feedback. This efficiency of adjustment within disciplines is to be found, too, in the curriculum; lecture courses in science rarely fail to take account of recent research. By general agreement training for the professions in Britain is at a very high standard. Universities are successful in producing graduates who have a mastery of the methodology and techniques within a discipline and who are equipped to become scientific workers or scholars. But in an era of mass higher education this is not what most graduates will become, and universities are certainly far less successful in matching their extrinsic goals to the circumstances of society. For its mission oriented function the homeostatic mechanism of the university is quite inadequate.

Changing the Aims

The goals of British universities have been changed before and there is mounting evidence that the time has come for new goals to be added to the old ones. It is a matter of history that in the past such major reappraisals have been imposed from outside, by competition from other universities or under the influence of royal commissions led by men such as R. B. Haldane.

The indications are that the present need for a change in goals of universities cannot be met by the minor adjustments in the present system, such as are being proposed, nor by pouring more money into the present system and allowing it to grow bigger. What is needed is a thorough revision of the inner logic of universities before the forces of pressure and suction overbalance them. To influence this revision is, in my view, likely to be a very important social function of science in the next decade, and perhaps the most effective way in which scientists in universities can exert their social responsibility.

The incongruence between the discipline oriented training which most undergraduates receive, and the mission oriented activities in which many of them wish to engage after graduation, is one of the causes of the present discontents in universities. In its shallow manifestations it takes the shape of demands for "relevance", by which the undergraduates may mean nothing more than instant recipes for solving social problems according to some preconceived political doctrine; but it is a justified discontent all the same, and if there is no response to it, that will be reinforcing the antiscience counter culture. Universities must, of course, continue to provide discipline oriented training for all those who aspire to be specialists, and to be implacably opposed to any dilution of this training. For this reason, if for no other, universities must remain centres of research. To segregate the best scientists in research institutes would be to cut them off from the best undergraduates: the line of succession would be severed. But it is a commonplace that the exponential phase of increase in basic scientific research is already over. It would be nonsense to assume that the volume of research must inevitably increase *pari passu* with the increase in student numbers, just because staff-student ratios do not change. Some research may have to be rationed. Indeed, the volume of research may already be above its final asymptotic level; it would be an interesting exercise to estimate how much pedestrian research is necessary in order to sustain the comparatively small volume of research of real significance.

But if the need for people to discover new science is diminishing, the need for people to combine science and common sense in the difficult art of technocratic decision making is likely to increase. Mass higher education must not produce armies of research workers; it should produce people who can integrate scientific and political considerations at all levels, from nuclear defence to the siting of a sewage plant. The parameters for this sort of decision making include scientific data (in a form which can be used for cautious prediction), estimates of practicability (it is no good making a decision which is stillborn), and a framework of principles (in the long run a nation holds in contempt those among its leaders whose decisions are based on mere expediency). One of the goals of universities should be to train people who can define the parameters and perform this sort of integration; for these are the people who will determine the choices of extrinsic goals for the deployment of science and technology.

This is a recent problem to which universities have, so far, given only trivial attention. I say "recent", because universities have not, in the past, been places devoted exclusively to rational and objective thought; they were religious foundations, and so were their analogues in the muslim world and in the orient.

Role of General Education

There are massive difficulties, the chief of which is a stubborn inertia within the system itself. This is illustrated by the way some academics have reacted to one of the very few constructive responses to the logistic problems facing universities, namely the proposal made originally by Pippard *et al.*⁸. The proposal is that undergraduates who enrol in faculties of science should be given a two year general education in science, regarded as "one of the arts and only peripherally as a technical skill", and leading to a bachelor's degree. Those who are competent and who wish to have a professional training in science (perhaps a third of the total) should pursue intensive courses in their specialism for another two years. There are other interesting features of the proposal, notably an easy system of transfer from one university to another, and between universities and polytechnics.

Pippard's proposals have two great merits. They could be achieved through a policy of accelerated evolution rather than through the hazardous alternative of revolution. And the proposals have enough degrees of freedom to permit further and more fundamental changes. If it is a social function of science to influence the determination of extrinsic goals for the deployment of science, the seeds for this expertise must be sown in universities. At the end of the two year course, students could elect to concentrate on discipline oriented studies (as the proposals intend) or on mission oriented studies. By these I mean not only the traditional ones, such as engineering and medicine, but studies in technocratic decision making; not at all on the lines of present courses in public administration, but by means of case history seminars on problems which require the integration of scientific, political and ethical principles. It is easy to think of topics for such seminars: the control of pollution in the Rhine (a most complex exercise in biology, law, economics, and international relations), urban living and transport, industrialization in tropical Africa and the political implications of computers, for example. In the control of population or traffic or pollution, millions of people have to participate; many of them, probably, unwillingly. These are problems in social engineering; they have a moral parameter, and as we no longer live in a society which prescribes rigid moral parameters, we have to determine them empirically (that is what much of the propaganda against pollution is about). A poor substitute for faith, some will say; but in a pluralistic society there is no better one.

And the teaching staff for these seminars? Of course, some of them would have to be imported part time into the university, and it would be necessary to persuade them and their employers, too, that this enterprise is not just a *hors d'oeuvre* as an appetizer to serious education, but is the main course of the education. The men who had to take part in decision making about the 300 GeV machine, the international conventions about oil discharges at sea, the siting of a third London airport, the rejuvenation of the Thames, would have to be mobilized to help. Alvin Weinberg has recently suggested⁹ that the pattern of the large mission oriented laboratories, such as Oak Ridge, Los Alamos and Goddard, should now be adapted for massive efforts to tackle the major social problems which disfigure existence and impair the quality of human life—race relations, the decay of cities, crime, and (he added) mass education. To exercise their full educational effect, these laboratories should be closely linked to universities or polytechnics.

This is one way in which the overhaul of goals for universities will have to go beyond Pippard's proposals. There are other ways—and again, the proposals are open-ended enough to permit of this. If the two year BA course is to be a success it will have to attract first class scientists as teachers. This requires a change in the criteria for esteem in the scientific world. In the world of art there is an honourable place for interpreters, but in science there is no comparable honour for the interpreter. If scientists do regard it as a social function

of science to educate generalists into the humane use of science, the interpreters of science to generalists must therefore be duly esteemed.

There is another contribution which scientists could make to this issue, namely to encourage rigorous research into the processes of higher education. Much educational research has, unfortunately, a deserved reputation for being futile and derisory. I think this is because unimaginative workers apply numerate techniques (such as chi-square tests to evaluate the influence of television on learning ability) to irrelevant problems, and they do not question their premises (we are, for instance, embarking on mass higher education still encumbered by Plato's theory of education for an élite in a slave state). When a man of distinction (for example, Piaget) applies his talents to this field, his work is very productive, and the Educational Testing Service at Princeton has published important work in this field¹⁰. It would help if more scientists of acknowledged distinction would give thought to the educational questions which ought to be asked about the mass higher education to which this nation is now committed. As a first step (for instance), every large science department might appoint one staff member to reflect in a sustained way about the goals of teaching the discipline of the department to those who will not become professionals, and, of course, to offer him reward and recognition for doing so.

One of the objections which is being raised to Pippard's proposals is that they would devalue the academic currency: all that they would do, in fact, is to revalue one coin in the currency. There are also fears that the two year BA would be a cheap product with a low market value, causing frustration among those who leave the university without proceeding to graduate work; this is a reasonable objection which would have to be met. It could be met first by a change in the attitude of employers, especially those in local and central government; they would have to be persuaded to accept the two year BA as the normal generalist qualification for administrative services and for parts of the teaching profession, on the understanding that, at some later date, it could be supplemented by further university study, for which they would be released on full pay. And secondly, there would have to be an "opportunity bank" which allowed students to delay taking up all or part of their grants for higher education until they felt ripe to take them, so that a student who left the university after two years could return five or ten years later, to take some discipline or mission oriented course. Indeed, if a degree, like a passport, expired after ten years, and was renewable only after reattendance at some place of higher education, we would have a built-in insurance against obsolescence. This would remove the disincentive to leave the university after only two years and would help to ensure that everyone at a university really wanted to be there. The new dynamic equilibrium between suction, pressure, and inner logic may be one in which universities, like museums and libraries, become places which people attend at any time throughout life when they have a reason for attending and which they leave, without dishonour or embarrassment, when their reason for attending is fulfilled. The sandwich course concept could last a lifetime.

¹ Brooks, H., *New York Times*, January 12 (1970).

² Bernal, J. D., *The Social Function of Science* (Watts, 1939).

³ Rose, H., and Rose, S., *Science and Society* (Penguin, 1970).

⁴ Roszak, T., *The Making of a Counter Culture* (Anchor, 1969).

⁵ MacKay, D. M., in *The Future of Man* (edit. by Wolstenholme, G.) (1963).

⁶ Zuckerman, S., *Beyond the Ivory Tower* (Weidenfeld and Nicolson, 1970).

⁷ Brooks, H., and Bowers, R., *Scientific American*, 222, 13 (1970).

⁸ Pippard, A. B., Parkes, E. W., Nicol, A. D. I., and Deer, W. A., *Nature*, 228, 813 (1970).

⁹ Weinberg, A., *Report to the Committee on Science and Astronautics* (US National Academy of Sciences, Washington, 1967).

¹⁰ Ashby, E., *Any Person, Any Study* (New York, 1971).

Atlantis Undiscovered — Bimini, Bahamas

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Underwater remains of an originally coherent formation of coquina limestone have been misinterpreted as an "enigmatic" stonework emplaced by prehistoric men. Marble and cement cylinders are found nearby. People who require the cylinders to be "pillar fragments" of an Olmec or Atlantean culture might consider a more mundane explanation.

IN 1969, J. M. Valentine described¹ what he called an "archaeological enigma" consisting of "pavement-like stones at 15 feet off North Bimini". Since then, newspaper reports, at least one magazine article² and two books³ have suggested, first, that there is a seawall or roadbed submerged at about 7 m off the north-west coast of North Bimini (Fig. 1) and, second, that sections of pillars which seem "to have been carved from natural stone" lie at shallow depths off Entrance Point. Last October an advertisement for one of the books, *Atlantis*, by R. Ferro and M. Grumley, appeared in the *New York Times*, confidently reporting that although ultimately it may turn out that Atlantis is no more than a legendary pot of gold . . . Ferro and Grumley discovered unmistakable traces of an ancient civilization—exactly where and when Edgar Cayce prophesied the re-emergence of Atlantis.

These occurrences have now been carefully investigated, using SCUBA gear, underwater cameras and hand tools. Most of the underwater work was done by Dr R. J. Byrne and Mr M. P. Lynch, who also helped in the interpretation of the data. We were guided to the sites by Mr Pino G. Turolla, said to have been the original discoverer^{1,3}.

The most obvious "pavement-like stones" or blocks form single or double lines roughly parallel to the present shoreline (Fig. 2A). The blocks here are between 60 and 90 cm thick, somewhat pillow-shaped in cross section, their originally right-angled corners having been trimmed back, chiefly by boring molluscs and sea urchins. All of the blocks are of coarse-grained limestone lying on a stratum of denser limestone of finer grain. Shifting sands cover this underlying formation in most places, giving the impression that the blocks have been placed there. Erosion at the interface of the two rock types has caused many of the largest blocks to fracture, either under their own weight or when storm swells have caused heaving and fracturing.

Although casual inspection of structures such as the fractured block of Fig. 2B might suggest small slabs that have been cut and fitted, closer examination of the opposing faces of the lifted and the unmoved pieces indicates an exact correspondence of bedding planes and surface morphology, so that all pieces are from the same original block. Similarly, the margins of adjacent large blocks correspond to one another, indicating that all blocks have developed by fracturing of an originally coherent formation. At no place are blocks found to rest on a similar set beneath. Samples of several blocks indicate that all are composed of shell-hash cemented by a blocky calcite, a type that originates only in the fresh water vadose or phreatic zones. The rock was thus almost certainly lithified during the lower relative sea level of the Pleistocene.

The geological setting of the blocks is important. The three small islands off Paradise Point are composed of a cemented wind-blown sand (eolianite) above a cemented shell-hash, with an interface roughly 1 m below low water. The beds of cemented shell-hash gravels and marine sands extend to at least 2 m below MLW, so that the sequence of blocks of coquina limestone overlying marine sand limestones to the north-east of the islands is not surprising.

The blocks are believed to have originated as follows. A shell-hash gravel was deposited in shallow water as relative sea level fell during the most recent emergence of the Bahama Banks, and later brought into the fresh water environment. The materials were cemented and joints formed in the material, as is usually the case with limestones⁴. After two sets of practically right angle joints had developed, submergence of the area brought the jointed coquina limestone first into the breaking zone of waves and then the offshore zones. Wave action probably caused much of the initial separation into blocks, but when the formation was farther offshore the destructive activity of marine life would have become dominant.

The overall result is a field of blocks that at first sight appear to have been fitted together, and this has led to state-

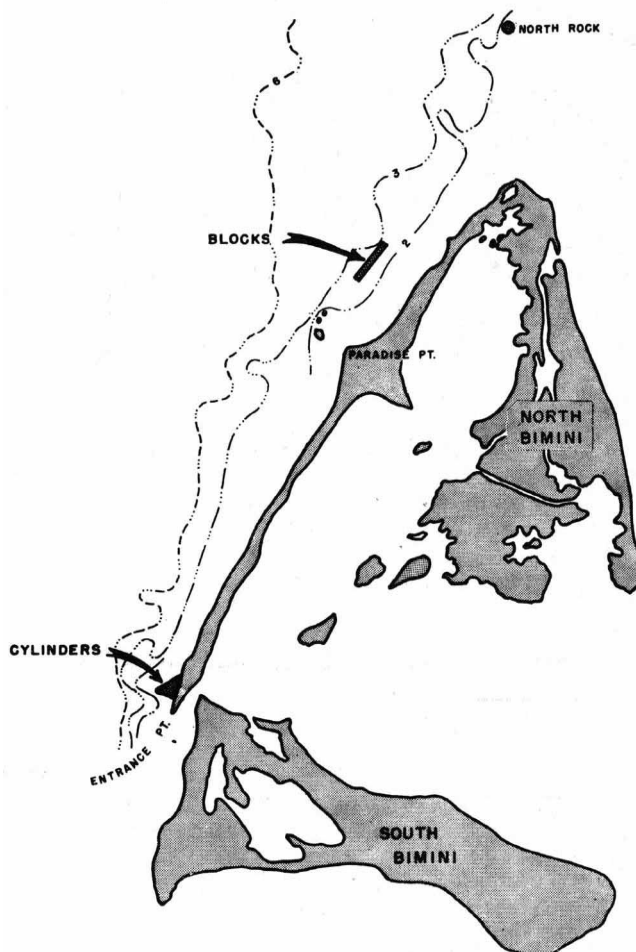


Fig. 1 Map of the Bimini Islands, showing locations of materials referred to in text. Bathymetric contours are in fathoms.

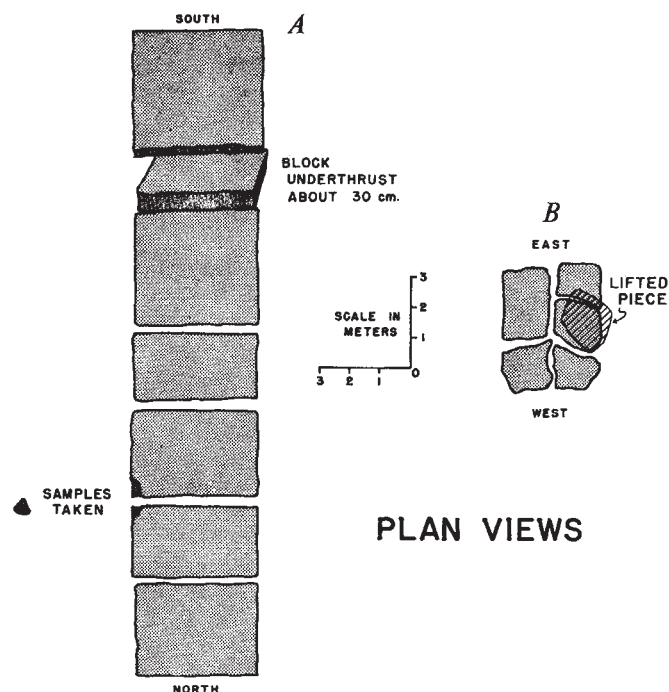


Fig. 2 Dimensions and arrangements of blocks (A) and fragments of a single fractured block (B).

ments¹ such as, (some) "human agency must have been involved". The blocky remains of the limestone outcrop are, however, no more enigmatic than other subaerial or subaqueous outcrops of jointed limestone found in various stages of fracture and decay in the north-western Bahamas.

The cylinders (Fig. 1), previously called "pillars", raise different considerations. They were found (Fig. 3) in grooves in the limestone country rock running roughly perpendicular to the present shoreline. Such grooves are common at Bimini⁵ and other Bahamian islands. Two of the cylinders are composed of marble and have flute-like marks parallel to the long axis. The wavelength of the crests is about 15 cm and their height about 1.5 cm. The remaining cylinders consist of what is most probably an early natural cement. All of the cylinders are encrusted with a layer of whitish CaCO_3 , a few mm thick. Although several of them have been attacked by boring molluscs and sea urchins, destruction has been hindered by periods of burial by sand.

Table 1 Eight Measurements of the Circumference of a 68.5 cm Long Cylinder, by 10 cm Intervals

Incremental distance (cm)	Circumference (cm)
0.0	135 (end)
10.0	152
10.0	160
10.0	165
10.0	163
10.0	157
10.0	147
8.5	142 (end)

Thin section examination of one of the marble samples indicates that it consists of calcite (90%) and quartz (8%) together with muscovite, pyrite and sphene. It is the metamorphosed equivalent of a calcite-rich limestone containing a small amount of clay. The marble is not native to the Bahamas, so that it would have had to be transported at least a few hundred miles to Bimini. Georgia is probably not the source and there is only a small chance that it could have come from Vermont (unpublished communication from J. B. Lyons). A possible clue to its origin is the pyrite content.

The cement cylinders are also composed of material which is not indigenous to the Bahamas (unpublished communication from R. Perkins). On balance, the material seems to be a hydrated natural cement.

Mr P. Klieger of the Portland Cement Association, Skokie, Illinois, has supported this opinion on the basis of X-ray and petrographic analysis, as has Dr R. C. Mielenz (Master Builders, Cleveland, Ohio) on the basis of petrographic analysis. Dr R. Nurse of the Building Research Station (England) has examined a thin section and concludes that it is a high temperature product and not an oxychloride cement and that "it resembles the 'grappier' made from the overburnt product of lime kilns".

Mrs Bryant Mather, US Army Corps of Engineers (unpublished communication), says that the material consists of "calcite, brucite, a complex calcium aluminium hydrate, quartz, hydrogarnet, a little ettringite and some sort of calcium aluminoferrite", suggesting that the material is a hydrated natural cement manufactured after about 1800. The material also contains widely separated particles of partially carbonized coal, supporting the belief that it is a simple natural cement from lime kilns in the United States, England, France or Belgium. Long-term action of seawater on the set material

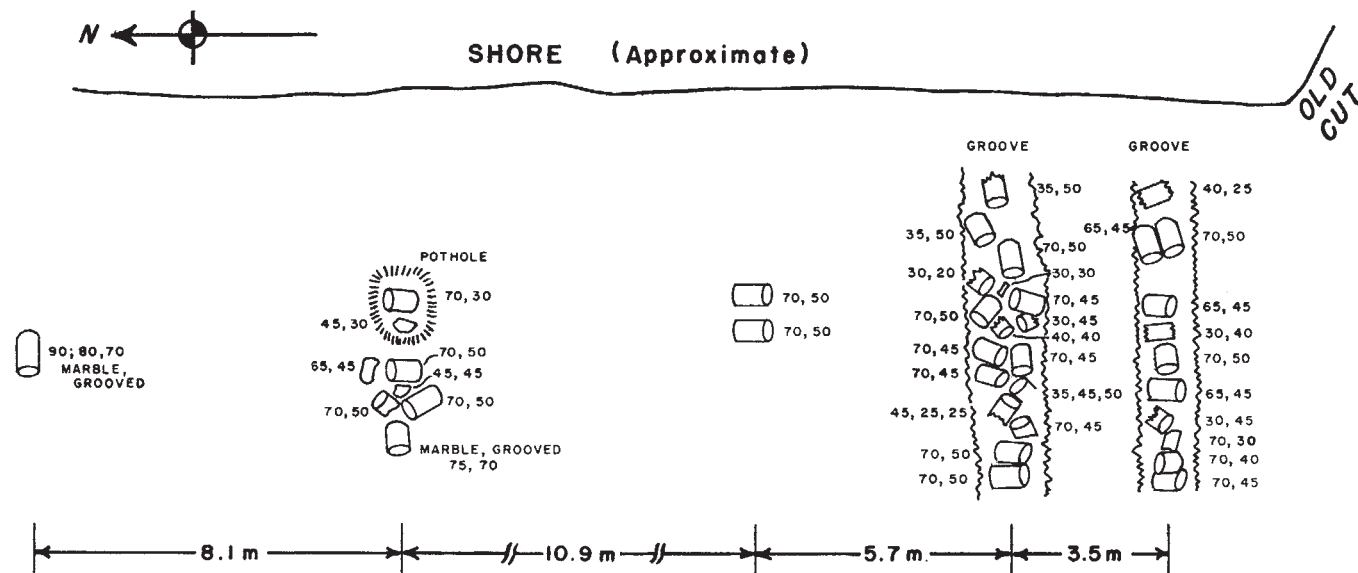


Fig. 3 Distribution of marble and cement cylinders off Entrance Point (April 17, 1970). Cylinders closest inshore were about 12 m from the shoreline. (First dimension is cylinder length in cm; the second is the diameter.)

would account for sulphate, chloride, and perhaps part of the magnesium, but an important proportion of the magnesium is an original constituent of the cement.

The most striking aspect of the cylinders is the constancy in size and shape of the whole ones. They are all barrel-shaped, about 70 cm long and 50 cm in diameter (Table 1). It seems most likely that the objects were formed by cement hardening in barrels or casks. The wooden containers would have by now been broken up and lost. The most likely explanation of the marble and cement cylinders is therefore that they are construction materials that were being transported by ship

when, either by shipwreck or design, they came to rest on the seafloor off Entrance Point.

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- ¹ Valentine, J. M., *Muse News*, 36 (1969).
- ² Marx, R. F., and Rebikoff, D., *Argosy* (December 1969).
- ³ Ferro, R., and Grumley, M., *Atlantis* (Doubleday, New York, 1970); Berlitz, C., *The Mystery of Atlantis* (Grosset and Dunlap, New York, 1969).
- ⁴ Newell, N. D., and Rigby, J. K., *Soc. Econ. Palaeontologists and Mineralogists, Spec. Pub.* 5, 1 (1957).
- ⁵ Newell, N. D., and Imbrie, J., *Trans. NY Acad. Sci.*, 2, 3 (1955).

Experimentally Created Incipient Species of *Drosophila*

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By means of selection in many generations, ethological (sexual) isolation has been built between strains of *Drosophila* which were formerly not reproductively isolated.

EXPERIMENTAL creation of new biological species by means of allopolyploidy—doubling the chromosome sets in interspecific hybrids—has been known for several decades. One of the classics in this field is the work of Karpechenko¹, who obtained a tetraploid *Raphanobrassica* (radicabbage) from hybrids between *Raphanus* (radish) and *Brassica* (cabbage). *Raphanobrassica* is almost fully fertile *inter se*, but forms highly sterile hybrids with the parental species. Some allopolyploid species existing in nature have been experimentally resynthesized; for example, the mint, *Galeopsis tetrahit*², bread wheats³ and others. Though widespread and important in some plant families, species formation by allopolyploidy is uncommon in the living world at large. A different mode of origin of species is prevalent among sexually reproducing and outbreeding organisms. This is accumulation of genetic differences between geographically separated (allopatric) populations or races, followed by a gradual emergence of reproductive isolation between them^{4,5}. Species formation through genetic divergence and its fixation by reproductive isolating mechanisms is a slow process, generally requiring many generations. For this reason, this kind of speciation has not been observed or reproduced in experiments. In a sense, we are in a situation today similar to that experienced by Darwin more than a century ago: differentiation of species is inferred from copious indirect evidence, but has not actually been observed. The experiments described here are therefore very unusual: what may be regarded as the crucial stage of the speciation process has taken place, and in part deliberately induced in laboratory experiments.

Superspecies *Drosophila paulistorum*

Drosophila paulistorum, a member of the *willistoni* species group of *Drosophila*, occurs in the American tropics, from Guatemala and Trinidad to southern Brazil. *D. paulistorum* is a compound of six semispecies or incipient species—genetically too different to be regarded as races of the same species, yet not different enough to be rated as six fully differentiated

species^{6,7}. It is difficult to cross the semispecies because of a pronounced ethological (sexual) isolation. No hybridization has been detected in nature. In the laboratory, when females and males of two semispecies are placed together, most of the matings occur within a semispecies. The exceptions, females of one semispecies inseminated by males of another, give progenies of apparently normal and vigorous hybrid daughters and sons. The hybrid females are fertile when backcrossed to males of either parental semispecies. The hybrid males are completely sterile.

The combination of these two isolating mechanisms permits sympatric coexistence of two, or even three, semispecies. Thus, the Central American semispecies occurs alone from Guatemala to Costa Rica and western Panama, but in central Panama it meets the Amazonian and Orinocan semispecies. The distribution of the Amazonian semispecies extends from Panama to Guiana, Rio Negro and the estuary of the Amazon (Belem). The Orinocan semispecies has been found in northern Colombia, Venezuela, Trinidad and Guiana. The interior semispecies occurs on upper Orinoco, Rio Negro, upper Amazon and their tributaries. The Andean semispecies occurs alone in central and southern Brazil, Bolivia, Peru and Ecuador, but it meets the Amazonian and Interior semispecies in central and southern Colombia, Trinidad, Venezuela, Guiana and along the Amazon and Rio Negro. Finally, the Transitional semispecies occurs on the Pacific Coast of Colombia, in northern Colombia and northern Venezuela⁷. The semispecies are indistinguishable morphologically but they can be distinguished cytologically^{8,9}; the easiest in practice and usually unambiguous method of recognition is outcrossing a strain to be diagnosed to six tester strains, one from each semispecies. Fertile hybrids are produced with only one of the six testers, that which represents the semispecies to which belongs the strain being tested.

Career of the Llanos Strain

A laboratory strain was established from a female captured on March 19, 1958, south of Villavicencio, in the Llanos of Colombia. The strain was classified as belonging to the Orinocan semispecies, because it crossed easily to, and gave fertile hybrids with, other Orinocan strains then available in the laboratory. The behaviour of the Llanos strain changed, however, between 1958 and 1963; in 1963 and thereafter, the male hybrids obtained from crosses of Llanos to Orinocan strains proved to be completely sterile. The possibility that the change in the behaviour of the Llanos strain might have resulted from accidental contamination was ruled out on two

grounds. First, a cytological study of the Llanos strain in 1959 showed two polymorphic chromosomal inversions which were, at that time, found in no other strain. Llanos still carries these chromosomal polymorphisms. Second, in 1963, Llanos was systematically outcrossed to all other strains of *D. paulistorum* then available in the laboratory, and gave fertile male hybrids with none. The reality of the change is not in doubt^{10,11}.

In 1964, Professor H. L. Carson sent us a strain established from a female collected at Marco, on the upper Amazon, Brazil. This strain gave fertile hybrids only with Llanos. Later, many other strains of the Interior semispecies, to which Marco belongs, were found in equatorial South America^{7,12}. Are we to conclude that the Llanos strain became transformed from a representative of the Orinocan semispecies to one of the Interior semispecies? Such a conclusion is not warranted, although we obviously do not know whether the Llanos strain would have given fertile male hybrids with the Interior semispecies in 1958 or 1959. What we do know is that although the Llanos strain has a pronounced sexual isolation from at least some Interior strains, it mates freely with the Orinocan strain. The isolation is fairly strong between Interior and Orinocan.

The lack of isolation between the Llanos and Orinocan strains was ascertained in 1964 with "male-choice" tests¹⁰. Virgin females of two strains, Llanos and an orange-eyed mutant found in an Orinocan strain from Georgetown, Guiana, were confined with males of one or the other of these strains. After about half the females had become inseminated, the females were dissected and the presence or absence of sperm in their seminal receptacles was determined under a microscope (see Table 1).

In tests of this kind, the "choice" is actually exercised by the females who accept some males in preference to others. Table 1 shows that Llanos males are accepted by females of their own kind more easily than by Orinocan females; Orinocan males are accepted equally by both kinds of females. In 1969, the experiments were repeated, using an observation chamber¹³. Virgin but mature females and males were introduced, in equal numbers, into a saucer-shaped chamber with a glass top, and the matings that occurred were recorded with the aid of a hand lens. To make the Llanos individuals distinguishable from the Orinocan, one or the other kind had one of their wings slightly clipped. Table 2 summarizes the observations made by Lee Ehrman¹³ on matings between the Llanos strain and an Orinocan strain derived from Georgetown, Guiana.

The isolation coefficient is not significantly different from zero. This coefficient is +1 if the isolation is complete because only matings between likes take place, and -1 if matings only between unlikes are observed. Tests of the mating preferences of Llanos flies with other strains of the Orinocan and Interior semispecies, made by S. Perez-Salas¹², gave the isolation coefficients shown in Table 3.

Slight, but sometimes statistically significant, preferences for mating within a strain are often found in experiments with *D. paulistorum* in which two strains of the same semispecies are derived from ancestors collected in different localities. The

Table 1 Inseminated (+) and Uninseminated (-) Females of Two Strains of *Drosophila paulistorum*, exposed to Males of One of These Strains

	Llanos ♀♀		Orinocan ♀♀	
	+	-	+	-
Llanos ♂♂	56	34	36	54
Orinocan ♂♂	44	47	44	48

Table 2 Matings observed between Llanos and Orinocan Strains

Llanos ♀ × Llanos ♂	Llanos ♀ × Orinocan ♂	Orinocan ♀ × Llanos ♂	Orinocan ♀ × Orinocan ♂	Isolation coefficient
34	22	23	23	+0.11 ± 0.10

Table 3 Isolation Coefficients of Crosses between Llanos and Other Strains

Llanos + Sarare, Orinocan	+0.33 ± 0.09
Llanos + Valparaiso, Interior	+0.18 ± 0.09
Llanos + Leticia, Interior	+0.12 ± 0.09
Llanos + Mitu, Interior	+0.24 ± 0.10
Llanos + Ocamo, Interior	+0.57 ± 0.08
Llanos + Ayacucho, Interior	+0.18 ± 0.12

Llanos strain crosses fairly easily with strains of both Orinocan and Interior semispecies, although the Interior semispecies are more strongly isolated ethologically from each other, and even more so from the other semispecies. We conclude that, having developed a sterility of male hybrids with the Orinocan semispecies, the Llanos strain did not acquire ethological isolation from the Orinocan. But can ethological isolation be superimposed on the hybrid sterility by means of artificial selection?

Selection for Isolation

Several workers¹⁴⁻¹⁶ have induced or intensified ethological isolation between strains of the same or of different species. The techniques used in all these experiments are identical in principle. Two strains between which the isolation is to be developed are made homozygous for different recessive mutant genes. Females and males of the two strains are placed together and they are allowed to mate freely and to produce offspring. The part of that offspring coming from matings between the strains will be wild type in phenotype, whereas the progeny of matings of females and males of the same strain will show the recessive mutant traits. The wild type flies are discarded, and the mutants are allowed to become parents of the next generation. The progenies of the flies that mate within a strain are thus included, and those of the matings between the strains are excluded from parentage. This imposes a selective advantage on genetic constitutions which favour matings within and discriminate against matings between strains.

The variant of this technique that had to be used in experiments with *D. paulistorum* is rather laborious. The Llanos strain has produced a sex linked recessive mutant, rough eye. An autosomal recessive mutant, orange eye, and sex-linked mutants, veinless wing and yellow body appeared in the Georgetown, Guiana, strain of the Orinocan semispecies. (Rough, orange and veinless were found by Mr B. Spassky, and yellow by O. P.) In October 1966, pairs of experimental populations were started according to the following design. Between fifty and a hundred (usually seventy) virgin females of Llanos rough mutant are placed in a culture bottle with males of the same strain and of one of the Orinocan mutant strains. In another culture bottle, fifty to a hundred Orinocan mutant females are exposed to a mixture of the same Orinocan mutant males and of Llanos rough eyed males. Among the progeny, the hybrid females, coming from matings of unlike parents, are distinguishable by being wild type in phenotype. Non-hybrid females, coming from matings of like parents, show the traits of the respective mutants. The hybrid females are discarded, and the non-hybrid ones used as parents of the next generation. Because rough, yellow and veinless are sex linked, hybrid sons of the mutant mothers are not distinguishable from the non-hybrids. This is inconvenient but not fatal for the experiments, because the hybrid males are completely sterile. Because orange is autosomal it permits discrimination of hybrid and non-hybrid females as well as males. Six pairs of experimental populations were made:

- 1A: rough ♀♀ + rough ♂♂ + orange ♂♂
- 1B: orange ♀♀ + orange ♂♂ + rough ♂♂
- 2A: rough ♀♀ + rough ♂♂ + veinless ♂♂
- 2B: veinless ♀♀ + veinless ♂♂ + rough ♂♂
- 3A: rough ♀♀ + rough ♂♂ + yellow ♂♂
- 3B: yellow ♀♀ + yellow ♂♂ + rough ♂♂

Populations 1A and 1B are at present (December 1970) in the seventy-third generation of selection. Populations 2A and 2B were discontinued after sixty-five generations, and 3A and 3B after sixty generations of selection. The proportions of the like and the unlike males to which the females were exposed were adjusted to supply in different generations the strongest challenge to the discriminating abilities of the females. At the same time, enough mutant progeny must be produced to serve as parents of the next generation. In the early generations, the two kinds of males were usually equally numerous; later the like males were less numerous than the unlike. The total numbers of males were about equal to the numbers of females.

The selection for ethological isolation was successful in all six populations, although in none has anything like complete isolation been achieved. It can be stated that, as a rule, the proportions of hybrids among the progenies have decreased from early generations to the later ones. The progress of the selection seems, however, very uneven. For example, the percentages of hybrid females obtained in every tenth generation in population 1A were: F_1 , 41.9; F_{10} , 17.3; F_{20} , 53.5; F_{30} , 31.8; F_{40} , 12.7; F_{50} , 13.9; F_{60} , 8.3; F_{70} , 31.8.

The unevenness is largely an artefact. It is caused in part by the inability mentioned earlier to distinguish the hybrid and the non-hybrid males with the sex-linked mutant rough; the proportions of fertile individuals among the rough-eyed male progenies in the different generations are therefore not exactly known. Another disturbing factor is that the rough, veinless, and yellow mutant markers considerably reduce the viability of their carriers. Only orange has a satisfactory viability. The hybrid females, being wild type, have an advantage in survival.

Ethological Isolation Observed

The proportions of hybrid offspring obtained in our experimental populations do not measure reliably the degree of the ethological isolation between the strains used. We accordingly used the observation chamber technique previously mentioned. Twelve females and twelve males of each of two strains (forty-eight flies in all) were introduced in a chamber without etherization. The chambers were observed for about 3 h, and the matings that took place were recorded. A female could mate only once during this time interval, whereas males were free to mate repeatedly. Our results are summarized in Table 4.

All the tests reported in Table 4 show statistically highly significant isolation coefficients, ranging from 0.55 ± 0.08 to 0.82 ± 0.05 . It should be recalled that unselected Llanos with unselected Orinocan shows little, if any, preferential mating (isolation coefficient $+0.11 \pm 0.10$, Table 2). Without doubt, the selection has developed an ethological isolation where perhaps only a trace of preference for mating within a strain existed before selection. It is furthermore remarkable that the tests in which one of the strains had been selected for isolation

and the other was unselected showed about as much isolation as did the tests where both strains had been selected (Table 4).

Status of the Llanos Strain

In 1958 and 1959, the Llanos strain was giving fertile hybrids with strains of the Orinocan semispecies. From 1963 on, it gave sterile hybrid males with the same strains. The reason for this change is uncertain. It may be that the change is related to the geographic origin of the Llanos strain. The region where it was collected lies between the known distribution areas of the Interior and Orinocan semispecies⁷. These semispecies seem more closely related to each other than to the remaining four semispecies. Nowhere do they coexist sympatrically although each of them does so with other semispecies. The genetic instability of the Llanos strain may have resulted from its being a form intermediate between, or even a hybrid of, Orinocan and Interior semispecies¹². Another possibility is that the change was brought about by an alteration in the population of mycoplasma-like intracellular symbionts, which seem to be different in the different semispecies^{17,18}.

At any rate, no appreciable reproductive isolation arose between the changed Llanos and Orinocan strains. Without ethological isolation, hybrids between them can be produced freely. The hybrids seem to be vigorous. Hybrid females are fertile, and so are the males in the offspring of the backcrosses. There is no barrier to gene flow. The gradual building up of ethological isolation changes the situation. The isolation coefficients listed in Table 4 are mostly below the mean, but well within the range of isolation coefficients encountered in experiments with semispecies found in nature— 0.28 ± 0.10 to 0.94 ± 0.03 (ref. 12). There are good reasons to think that ethological isolation is probably stronger in nature than under laboratory conditions. This makes some of the semispecies able to coexist in the same territory, sympatrically, without mixing. Orinocan and Interior semispecies are an exception—they are not sympatric. Isolation coefficients between strains of these semispecies obtained in laboratory experiments range from 0.28 ± 0.10 to 0.65 ± 0.08 (ref. 12), lower than we achieved (Table 4).

We conclude that the selected Llanos strain is comparable with the naturally existing semispecies. It could not, however, maintain itself if it were sympatric with the Orinocan semispecies. To render artificial selection possible, the Llanos strain was made homozygous for the mutant gene rough eye. This mutant reduces the viability of its carriers, whereas the hybrids with Orinocan, which have the rough eye suppressed by its dominant normal allele, are more vigorous. Natural selection for ethological isolation would occur if the hybrids were, on the contrary, at a disadvantage. The last step needed to make the Llanos semispecies capable of sympatric coexistence with the Orinocan semispecies is to free it from the rough-

Table 4 Observed Matings between Selected and Unselected Strains of *Drosophila paulistorum*

Date	Strains		Matings				Isolation coefficient
	A	B	$A\text{♀} \times A\text{♂}$	$A\text{♀} \times B\text{♂}$	$B\text{♀} \times A\text{♂}$	$B\text{♀} \times B\text{♂}$	
March 1970	Llanos U	Orange S	41	13	11	40	0.55 ± 0.08
March 1970	Rough S	Orinocan U	47	17	6	40	0.59 ± 0.08
October 1969	Rough S	Orange S	52	4	4	41	0.67 ± 0.07
June 1970	Rough S	Orange S	45	7	3	54	0.82 ± 0.05
March 1970	Llanos U	Veinless S	60	2	12	42	0.76 ± 0.06
October 1969	Rough S	Veinless S	43	8	10	41	0.64 ± 0.08
June 1970	Rough S	Veinless S	44	8	11	45	0.64 ± 0.07
March 1970	Llanos U	Yellow S	48	8	13	43	0.62 ± 0.07
October 1969	Rough S	Yellow S	50	6	13	32	0.62 ± 0.08
June 1970	Rough S	Yellow S	27	16	4	66	0.65 ± 0.07

S, Selected; U, unselected.

The designation Llanos unselected refers to the wild type Llanos strain which had not been exposed to challenges of hybridization with Orinocan strains; rough selected is the rough-eyed mutant which had been so exposed for fifty generations by October 1969, and fifty-eight generations by March 1970; Orinocan unselected is the wild type Orinocan strain from Georgetown, Guiana; orange selected, veinless selected and yellow selected are the three mutants which arose in the Georgetown strain, and were selected for as many generations as their rough selected counterparts.

eye mutant, without disturbing the rest of its genotype responsible for the ethological isolation. This may not be easy to achieve, but it can be attempted.

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¹ Karpechenko, G. D., *Z. Indukt. Abstamm.- u. Vererbungslehre*, **48**, 1 (1928).

² Müntzing, A., *Hereditas*, **16**, 105 (1932).

³ McFadden, E. S., and Sears, E. R., *J. Hered.*, **37**, 81, 107 (1946).

⁴ Mayr, E., *Animal Species and Evolution* (Belknap, Cambridge, 1963).

⁵ Dobzhansky, Th., *Genetics of the Evolutionary Process* (Columbia University Press, New York and London, 1970).

⁶ Dobzhansky, Th., and Spassky, B., *Proc. US Nat. Acad. Sci.*, **45**, 419 (1959).

⁷ Spassky, B., Richmond, R. C., Perez-Salas, S., Pavlovsky, O., Mourão, C. A., Hunter, A. S., Hoenigsberg, H., Dobzhansky, Th., and Ayala, F. J., *Evolution* (in the press).

⁸ Kastritsis, C. D., *Chromosoma*, **23**, 180 (1967).

⁹ Kastritsis, C. D., *Evolution*, **23**, 663 (1969).

¹⁰ Dobzhansky, Th., and Pavlovsky, O., *Proc. US Nat. Acad. Sci.*, **55**, 727 (1966).

¹¹ Dobzhansky, Th., and Pavlovsky, O., *Genetics*, **55**, 141 (1967).

¹² Perez-Salas, S., Richmond, R. C., Pavlovsky, O., Kastritsis, C. D., Ehrman, L., and Dobzhansky, Th., *Evolution*, **24**, 519 (1970).

¹³ Ehrman, L., *Evolution*, **19**, 459 (1965).

¹⁴ Koopman, K. F., *Evolution*, **4**, 135 (1950).

¹⁵ Knight, G. R., Robertson, A., and Waddington, C. H., *Evolution*, **10**, 14 (1956).

¹⁶ Kessler, S., *Evolution*, **20**, 634 (1966).

¹⁷ Williamson, D. L., and Ehrman, L., *Genetics*, **55**, 131 (1967).

¹⁸ Kernaghan, R. P., and Ehrman, L., *Chromosoma*, **29**, 291 (1970).

Directed Genetic Change Model for *X* Chromosome Inactivation in Eutherian Mammals

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Random inactivation of one or other of the *X* chromosomes in female eutherians could have evolved from an ancestral paternal *X* inactivation, which has been retained in marsupials.

In 1961 Lyon put forward the single active *X* hypothesis to account for dose compensation of genes on the sex chromosomes of female eutherian mammals¹. Under her hypothesis in the adult organism either the maternally or the paternally derived *X* is active in any one cell, but never both. The question of which *X* is to be inactivated is settled at a very early embryonic stage²⁻⁴. In female diploid individuals there is usually something near a 1 : 1 ratio of cells with active paternal to cells with active maternal *X* chromosomes⁵. The inactive *X* is detected cytologically as the heterochromatic Barr body or sex chromatin in interphase nuclei and because it synthesizes its DNA late, that is, it labels late⁵. Once established the inactivation is very stable. In abnormal individuals with more than two sex chromosomes all *X* chromosomes except one synthesize their DNA late and form Barr bodies⁶. Evidence from the phenotype of women with abnormal numbers of *X* chromosomes or *X* chromosomes with deletions suggests that the second *X* has some active genes⁷. With these exceptions, it can be said that cytological observations and studies on a number of sex linked genes have yielded considerable data supporting Lyon's hypothesis and none definitely against it^{5,8,9}.

Marsupial mammals are the closest extant group to eutherians¹⁰. Their system of sex determination is *XX/XY*^{11,12} with the *Y* being male determining^{12,13}. They have a late labelling *X*^{11,14,15}. Sharman has obtained evidence that in kangaroos the late labelling *X* is always paternal in origin¹⁴, that is, there is paternal rather than random *X* inactivation, a hypothesis which is supported by data on the inheritance of the enzymes glucose-6-phosphate dehydrogenase (G6PD)¹⁶ and phosphoglycerate kinase (PGK)¹⁷. On the basis of this finding I wish to

propose a model of *X* inactivation in eutherians. The model postulates that random *X* inactivation in eutherians has evolved from an ancestral paternal *X* inactivation which has been retained in marsupials. It proposes that during male meiosis in both groups the *X* undergoes a directed genetic change. A controlling element, analogous to the kind described by McClintock in maize¹⁸, is introduced into the *X*, probably by the *Y*. Then at a certain point in the very early development of eutherian females this controlling element is excised and later reinserted at random into one of the two chromosomes, thus setting up random *X* inactivation. Once reinserted, it remains fixed in that chromosome. The purpose of this article is to examine the adequacy with which the model accounts for data at present available and to propose new investigations which should test it more critically.

Maize Controlling Elements

Controlling elements may be exemplified by the activator-dissociator (*Ac-Ds*) system of maize¹⁸. This system is in many respects like an operon system in which both the "regulator" element (*Ac*) and the "operator" element (*Ds*) have the episome-like property of occasionally being able to move from one part of the genome to another (although there is no evidence that either is capable of autonomous existence in the cytoplasm). Controlling element is the term used to describe entities like *Ac* and *Ds*, in contradistinction to gene which is reserved for conventional Mendelian units. Apart from being capable of this occasional transposition, controlling elements segregate at meiosis in the normal way. When the *Ds* element is present at a particular locus, genes adjacent to it on the same chromosome have their activity suppressed to some degree. Their expression is said to be unstable. Action of *Ds* depends on the existence of one or more *Ac* elements in the genome, not necessarily on the same chromosome or its homologue. *Ac* also affects the activity of genes adjacent to it in the chromosome. The occasional transposition of *Ac* and *Ds* probably occurs during the replication cycle of the chromosome. The genes adjacent to the old location usually return to normal activity while those adjacent to the new locus become unstable

in their expression. Removal, however, is often associated with the appearance of a new stable allele of the adjacent gene. The *Ac-Ds* system has a number of other peculiarities, only one of which need concern us here. *Ds* may undergo mutation-like events which McClintock terms "changes of state". This term serves to describe the fact that the initial response of *Ds* to *Ac* may differ from subsequent ones. For example, when first isolated *Ds* responded to *Ac* by promoting the formation of dicentric chromatid bridges (a property from which *Ds* derives its name). In further responses, these were chiefly absent and only activity of adjacent genes was affected.

Controlling Element of Mouse *X*

Several workers have studied a region of the mouse *X* called the inactivation centre, which governs the degree to, or the frequency with, which the translocated autosomal material is inactivated when the associated *X* material is inactivated as a consequence of random *X* inactivation^{19, 21}. The inactivation of *X*-linked genes is likewise under control of this region of the *X*^{22, 23}. Cattanach calls the inactivation centre a controlling element, because there is an indication from his work on the flecked (*X^T*, *T(1X)*, *Ct*) *X* autosome translocation that it behaves like a maize controlling element^{19, 24}. This translocation involves the insertion of autosomal material from linkage group 1 into the *X*, making it about 20% larger^{19, 25}. The inserted material bears genes from albino (*c*) to ruby eye (*ru-2*). The locus of insertion is near jimpy (*jp*) (Fig. 1), but whether on the side distal or proximal to Gyro (*Gy*) is not yet known. Cattanach's early data indicated change from one generation to the next in the amount of inactivation of the autosomal material in the *X^T* chromosome which could not all be satisfactorily explained by meiotic crossing over between *X^T* chromosomes governing different levels of inactivation²⁴. He proposed instead that the controlling element was undergoing "changes of state". In a more recent investigation, the results obtained could all be explained by meiotic crossing over, but whether these later results render the change of state interpretation of the earlier results invalid is doubtful¹⁹. (There is some discussion as to how many controlling elements exist in the inactive *X* of the mouse^{8, 19}. Since only one region of the chromosome has so far been clearly implicated as being a controlling element, I will assume for the purposes of this paper that there is only one.)

Directed Change and Primary Paternal Inactivation

Cattanach and Perez have made an observation which suggests that the properties of the paternal *X* may differ from those of the maternal²⁶. They have recently shown that female mice heterozygous for *X^T* chromosomes tend to have lower levels of variegation for albino when the rearranged *X* is inherited from the father rather than the mother. A maternal effect is not responsible for the differences between reciprocal crosses. This can be explained by postulating that a directed genetic change at male meiosis alters the properties of the *X^T*. As a consequence, the *X^T* inherited from the father associates less frequently with the controlling element.

If the primary step in establishing random *X* inactivation is paternal *X* inactivation, there should be mutant genes which convert the former to the latter. These genes may be defective controlling elements which cannot be transferred or genes governing enzymes mediating their transfer. There is some evidence which can be interpreted to mean that such genes do exist, the chief of which involves Searle's translocation. This, designated T16H, is a reciprocal *X* autosome translocation in the mouse²⁷⁻³⁰. With the exception of Cattanach's translocation, all mouse *X* autosome translocations render the male sterile. Hence all females heterozygous for Searle's translocation receive their rearranged chromosomes from their mother and their normal *X* and autosomes from their father. Since the

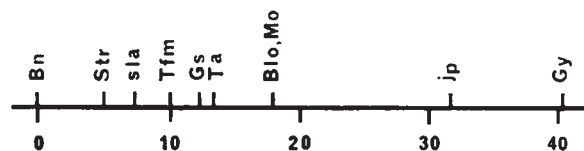


Fig. 1 Diagram of portion of the mouse *X* linkage group, showing position of sex linked genes discussed in the text. The diagram is from ref. 48. Bn, Bent tail; Str, striated; sla, sex linked anaemia; Tfm, testicular feminization; Gs, greasy; Ta, tabby; Blo, blotchy; jp, jimpy; Gy, gyro. The units are centimorgans.

normal *X* is almost always inactive in these heterozygotes, Searle's translocation exhibits paternal *X* inactivation. In this respect it is unlike all other mouse *X* autosome reciprocal translocations, which by contrast show random inactivation of either the normal or rearranged chromosome.

It is particularly significant that the break in Searle's translocation maps is the same region of the *X* as the controlling element. The break in the *X* is given as 0.85 centimorgans from Ta on the side distal from Blo (ref. 30 and Fig. 1). But the precise relationship of the breakpoint to genes in this part of the linkage group remains obscure because the translocation evidently leads to some cross-over suppression³¹. Cattanach *et al.* have concluded that their controlling element lies very close to Ta¹⁹. The data of Grahn *et al.* suggest that the controlling element may be 8.2 centimorgans from Gs on the side distal from Ta²¹. Problems of classification have not been completely resolved in their material and so this mapping is tentative. At present there seems to be nothing against the suggestion of Cattanach *et al.* that the break in Searle's translocation may be in the controlling element, thus impairing its function¹⁹. In my model, its transfer to the maternal *X* is prevented.

An alternative explanation of non-random inactivation in Searle's translocation is that the translocated *X* segment can only be switched off for the segment containing the controlling element. Switching off of the re-arranged *X* would lead to the breakdown of dosage compensation for genes on the segment lacking the controlling element. Selection of cells with correct dose compensation would then account for the observed preponderance of cells with inactive normal chromosomes⁵. The finding that at least one and probably two other mouse *X* autosome translocations with break points near Ta show random *X* inactivation makes this explanation unlikely²⁰.

There are other examples of apparent or possible paternal *X* inactivation in eutherians. Studies on the G6PD of mules indicate that in most tissues there is preferential inactivation of the paternal donkey chromosomes^{32, 33}. This is interpreted here to mean that the lack of homology between the two distantly related *X* chromosomes hinders the incorporation of the controlling element into the maternal *X*. An *X* autosome translocation heterozygote with preferential late labelling of the normal *X* has been described in the domestic cow, without any information on the parental origin of the arrangement³⁴. The explanation given for the behaviour of Searle's translocation is, of course, also applicable to this.

Unsolved Problems

The model advanced explains how random *X* inactivation evolved, how the choice of chromosome to be inactivated is made, its stability once made, and a number of hitherto unexplained aspects of *X* inactivation, particularly the behaviour of Searle's and Cattanach's translocations. It cannot, however, represent the whole truth, because several difficult questions remain unanswered.

How can XO individuals in man and mouse be viable when their *X* is from the father, as it often can be³⁵⁻³⁷? Obviously the controlling element must somehow be jettisoned or inactivated very early in development if the model is correct. But why should this happen to the paternal *X* in XO individuals but not in normal females?

Only one X is active in women and men with multiple X chromosomes⁶. Where do the extra controlling elements come from in cases where the extra chromosomes are from the mother³⁷? Perhaps the controlling element is retained during female meiosis and is included in a polar body, except when non-disjunction occurs leading to a multiple X gamete. This will explain the human data except for situations where four X chromosomes come from the mother, as in some $XXXXY$ and $XXXX$ individuals³⁷. For these female germ line polysomy must be invoked.

Retention of the controlling element cannot occur in the mouse, however; Searle's translocation gives normal segregation ratios for genes near the controlling element locus³¹, which it would not do if the chromosome with the controlling element was preferentially included in a polar body. The only report of multiple X individuals in the mouse concerns XXY types, which were the result of fertilization of a normal ovum by an XY sperm³⁸. It is thus of some importance to my model to discover if more than one X can be inherited from the maternal parent in a viable mouse.

Other problems are posed by the fact that in women heterozygous for a normal X and either Xqi (isochromosomes for the long arm of the X), Xpi (isochromosomes for the short arm), $Xq-$ (lack of a long arm), $Xp-$ (lack of a short arm) or Xr (a ring X) invariably have the abnormal X late labelling, irrespective of whether the abnormal X is paternal or maternal in origin^{37,39,40}. The only exception to this rule seems to be one report of an XXr individual with a late labelling normal X ⁴¹. How does the controlling element select the abnormal X ? And if Xqi , Xpi , $Xq-$, and $Xp-$ can all be inactivated, where is the locus of the controlling element? Must it be at or near the centromere in man?

Some explanation is needed to account for situations where the second X is apparently active. In marsupials these are the ovaries and uterus of the early pouch young kangaroo¹⁷ and possibly the ovaries of bandicoots¹¹. In placental mammals, oocytes⁵ and the early embryo^{5,42-44} lack sex chromatin. Possibly the controlling element is not fixed in either X at these stages. Or alternatively the controlling element could act in adult somatic tissue in an operator-like manner as the site of action of a repressor protein, which is absent at these early stages. In this connexion, one may note that Steele⁴⁵ has recently shown that human female foetuses and newborn female infants have more G6PD activity than males of either class, while hypoxanthine guanine phosphoribosyl transferase was essentially the same in both sexes at these stages. The structural loci for both enzymes are sex linked. In this instance induction of greater activity of the one active X seems more likely than switching on of the second X .

Possible Experimental Tests

Although it cannot explain all data on X inactivation, the model explains sufficient to make worthwhile investigations to test its chief assumptions, namely, directed genetic change of the X by the Y being the primary step in setting up random X inactivation followed by transfer of the inactivation to the maternal X . If these are correct, two kinds of mutation should exist. There should be mutations in the gene on the Y responsible for the directed genetic change. Males carrying them would give rise only to males. If the mutation is found in the mouse, matings with XO mice will give both male and female offspring, the latter being all XO . There should also be mutations which affect the transfer of the controlling element, and which will often result in paternal X inactivation. Such genes could be either X -linked or autosomal. Hopefully both sexes carrying them will be fertile, so that reciprocal crosses can be made to establish their nature rigorously, something which cannot be done with Searle's translocation. It is also possible that genes converting random X inactivation to paternal X inactivation are normally present in some eutherian species. Conversely, it is possible but rather less likely that there are

marsupials which, unlike kangaroos, possess random X inactivation. There is a need for a large survey of both marsupial and eutherian mammals to detect more sex-linked enzyme polymorphisms. So far no exceptions have been discovered to Ohno's thesis^{5,46} of homology between the sex chromosomes of all eutherian and marsupial X chromosomes. Hence the best tactic in a search for sex-linked isoenzymes is to use those enzymes known to be sex-linked in man, for example, G6PD⁴⁶ and PGK⁴⁷, a procedure which is now being followed in this laboratory.

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- ¹ Lyon, M. F., *Nature*, **190**, 372 (1961).
- ² Axelson, M., *Hereditas*, **60**, 347 (1968).
- ³ Austin, C. R., *The Sex Chromatin* (edit. by Moore, K. L.), 241 (Saunders, Philadelphia, 1966).
- ⁴ Issa, M., Blank, C. E., and Atherton, G. W., *Cytogenetics*, **8**, 219 (1969).
- ⁵ Ohno, S., *Sex Chromosomes and Sex-Linked Genes* (Springer, Berlin, Heidelberg, New York, 1967).
- ⁶ Barr, M. L., *The Sex Chromatin*, 129 (edit. by Moore, K. L.) (Saunders, Philadelphia, 1966).
- ⁷ Hamerton, J. L., *Nature*, **219**, 910 (1968).
- ⁸ Lyon, M. F., *Ann. Rev. Genet.*, **2**, 31 (1968).
- ⁹ Lyon, M. F., *Phil. Trans. Roy. Soc.*, **B**, **259**, 41 (1970).
- ¹⁰ Simpson, G. G., *Evolution*, **13**, 405 (1959).
- ¹¹ Hayman, D. L., and Martin, P. G., *Comparative Mammalian Cytogenetics*, 191 (edit. by Benirschke, K.) (Springer, New York, 1969).
- ¹² Sharman, G. B., *Science*, **167**, 1221 (1970).
- ¹³ Sharman, G. B., Robinson, E. S., Walton, S. M., and Berger, P. J., *J. Reprod. Fert.*, **21**, 57 (1970).
- ¹⁴ Sharman, G. B., *Nature*, **230**, 230 (1971).
- ¹⁵ Marshall-Graves, J. A., *Exp. Cell. Res.*, **46**, 37 (1967).
- ¹⁶ Richardson, B. J., Czuppon, A., and Sharman, G. B., *Nature New Biology*, **230**, 154 (1971).
- ¹⁷ Cooper, D. W., VandeBerg, J. L., Sharman, G. B., and Poole, W. E., *Nature New Biology*, **230**, 155 (1971).
- ¹⁸ McClintock, B., *Brookhaven Symp. Biol.*, No. 18, 162 (1965).
- ¹⁹ Cattanaach, B. M., Perez, J. N., and Pollard, C. E., *Genet. Res.*, **15**, 183 (1970).
- ²⁰ Russell, L. B., and Montgomery, C. S., *Genetics*, **64**, 281 (1970).
- ²¹ Grah, D., Leu, R. A., and Hulesch, J., *Genetics*, **64**, 2 (2) s25 (abstr.) (1970).
- ²² Cattanaach, B. M., *Genetics*, **60**, 168 (1968).
- ²³ Cattanaach, B. M., Pollard, C. E., and Perez, J. N., *Genet. Res.*, **14**, 223 (1969).
- ²⁴ Cattanaach, B. M., and Isaacson, J. H., *Genetics*, **57**, 331 (1967).
- ²⁵ Cattanaach, B. M., *Genet. Res.*, **8**, 253 (1966).
- ²⁶ Cattanaach, B. M., and Perez, J. N., *Genet. Res.*, **15**, 43 (1970).
- ²⁷ Searle, A. G., *Hereditas*, **17**, 297 (1962).
- ²⁸ Ford, C. E., and Evans, E. P., *Cytogenetics*, **3**, 295 (1964).
- ²⁹ Ohno, S., and Lyon, M. F., *Chromosoma*, **16**, 90 (1965).
- ³⁰ Lyon, M. F., *Genet. Res.*, **7**, 130 (1965).
- ³¹ Lyon, M. F., Searle, A. G., Ford, C. E., and Ohno, S., *Cytogenetics*, **3**, 306 (1964).
- ³² Giannelli, F., Hamerton, J. L., Dickson, J., and Short, R. V., *Hereditas*, **24**, 175 (1969).
- ³³ Hook, E. B., and Brustman, L. D., *Genetics*, **64**, 2 (2), s30 (abstr.) (1970).
- ³⁴ Gustavsson, I., Fraccaro, M., Tiepolo, L., and Lindsten, J., *Nature*, **218**, 183 (1968).
- ³⁵ Cattanaach, B. M., *Genet. Res.*, **12**, 125 (1968).
- ³⁶ Morris, T., *Genet. Res.*, **12**, 125 (1968).
- ³⁷ Race, R. R., and Sanger, R., *Brit. Med. Bull.*, **25**, 99 (1969).
- ³⁸ Cattanaach, B. M., *Genet. Res.*, **2**, 156 (1961).
- ³⁹ Klinger, H. P., Lindsten, J., Fraccaro, M., Barrai, I., and Dolinar, Z. J., *Cytogenetics*, **4**, 96 (1965).
- ⁴⁰ Rowley, J., Muldal, S., Lindsten, J., and Gilbert, C. W., *Proc. US Nat. Acad. Sci.*, **51**, 779 (1964).
- ⁴¹ Pfeiffer, R. A., and Buchner, T., *Nature*, **204**, 804 (1964).
- ⁴² Austin, C. R., in *The Sex Chromatin*, 241 (edit. by Moore, K. L.) (Saunders, Philadelphia, 1966).
- ⁴³ Kinsey, J. D., *Genetics*, **55**, 337 (1967).
- ⁴⁴ Hill, R. N., and Yunis, J. J., *Science*, **155**, 1120 (1961).
- ⁴⁵ Steele, M. W., *Nature*, **227**, 496 (1970).
- ⁴⁶ Ohno, S., *Ann. Rev. Genet.*, **3**, 495 (1969).
- ⁴⁷ Valentine, W. N., Hsieh, H. S., Paglia, D. E., Anderson, H. M., Baughman, M. A., Jaffe, E. R., and Garson, O. M., *Trans. Assoc. Amer. Phys.*, **81**, 49 (1968).
- ⁴⁸ Hawkes, S. G., *Mouse News Letter*, **43**, 16 (1970).

LETTERS TO NATURE

PHYSICAL SCIENCES

Refractive Index of Aqueous HCl Solutions and the Composition of the Venus Clouds

PROGRESS in the observation and theoretical interpretation of polarized reflected sunlight has led to a precise knowledge of the refractive index, particle size and shape, and altitude of the top of the Venus clouds¹⁻³. Hansen concluded that the cloud particles are spherical liquid droplets of extremely uniform size, with mean radii very near 1 μm , with a refractive index of 1.45 ± 0.02 near $\lambda = 0.55 \mu\text{m}$. These particles are found near the 50 mbar pressure level, compared with the usual estimates of 150 mbar for the "line-forming region" in which multiply scattered infrared photons undergo the most absorption and where the clouds become opaque enough to limit deeper penetration of sunlight. The temperature at the 150 mbar level is near 240 K. Theoretical calculations on the atmospheric structure above this level suggest a temperature possibly as low as 180–210 K at the 50 mbar level⁴.

The polarization measurements reflect the scattering histories of photons which have experienced relatively few scattering events, and thus contain information about either the very top of the main cloud layer or a rather dense high-altitude haze layer.

Several suggestions have been made for the chemical composition of the topmost clouds, including two liquids with refractive indices in the range 1.4–1.5. These are carbon suboxide⁵ (C_3O_2) and aqueous HCl solutions^{6,7}.

The refractive index of C_3O_2 has been measured at three temperatures at a wavelength of 5890 Å (the Fraunhofer D line)⁸. At 273.2 K, $n_D = 1.4538$; at 271.9 K, $n_D = 1.4596$; and at 261.2 K, $n_D = 1.4676$. A very approximate value for the temperature coefficient of the refractive index is $-0.00115 \text{ deg}^{-1}$. Clearly, even a short extrapolation to 250 K gives a refractive index of 1.480, already outside the error limits given by Hansen (unpublished work) for the Venus clouds.

Detailed data on the concentration dependence of the refractive index of HCl solutions are available for 291 K⁹, 298 K¹⁰ and 303 K¹⁰, and it is possible to derive approximate temperature coefficients for the refractive index. There are also data on the refractive index at 4860 Å, the Fraunhofer F line, and for a wide range of solution concentrations at 291 K⁹. Fig. 1 presents that portion of the observed values of n_D for HCl concentrations under 25% by weight of 5.5 M HCl for the purpose of documenting the observed temperature effect. The temperature effect can be fitted well by the equation

$$dn_D/dT = 8.3 \times 10^{-5} + 1.67 \times 10^{-5} C - 8 \times 10^{-7} C^2$$

where C is the molarity of the HCl solution. A calculation of n_D for a 24.8% by weight HCl solution (eutectic composition) at 200 K (13° above the eutectic temperature) gives $n_D = 1.406$. Schreiner's data on n_D and n_F of identical solution samples⁹ give a wavelength effect of

$$n_F = n_D + 0.0042 + 0.0024 C$$

We can then estimate that for a eutectic composition solution at 200 K, $n_F = 1.413$. Because of the strong concentration effect, at 200 K and 24.6% by weight HCl, $n_F = 1.421$.

The extrapolations involved are rather long, however, and any other source of data regarding the refractive index at low temperatures would be most useful.

Precise data on the density of cold, concentrated HCl solutions are available, and the refractive index can be calculated on secure theoretical grounds. The specific refraction, $P_{\lambda,c}$, can be calculated for each solution concentration for which data are available at both the Fraunhofer D and F lines. By means of the Lorentz-Lorenz equation,

$$\frac{n_{\lambda,c}^2 - 1}{n_{\lambda,c}^2 + 2} \frac{1}{\rho_{\lambda,c}} = P_{\lambda,c}$$

which relates the constant specific refraction to the refractive index and density of solutions of fixed concentration, the available density data can be used as a function of temperature to produce the refractive index of very cold solutions, the refractive indices of which have not been measured. The Lorentz-Lorenz equation has been widely used for both pure substances and constant-composition solutions, and can be confidently applied in the present case.

We have checked the accuracy of our extrapolations of the observed temperature dependence of the refractive index by using the Lorentz-Lorenz equation with the solution density data of Garrett *et al.*¹¹ to calculate n_D and n_F for a eutectic solution at 200 K. We find 1.408 and 1.418, respectively, compared with the crude extrapolations of 1.406 and 1.413 reported here. Similarly, we find n_F for a 27.6% by weight HCl solution at 200 K to be 1.428, compared with the estimate of 1.421 reported above.

The HCl-H₂O equilibrium phase diagram⁷ shows that 27.6% by weight HCl freezes partially, with formation of solid $\text{HCl} \cdot 3\text{H}_2\text{O}$, at 213 K, but this does not preclude such a supercooled solution, because Garrett *et al.* were easily able to cool down a 27.6% HCl liquid sample to 198 K. The supercooling of liquids and solutions in the absence of appropriate freezing nuclei is quite common and is abundantly documented in the terrestrial meteorological literature.

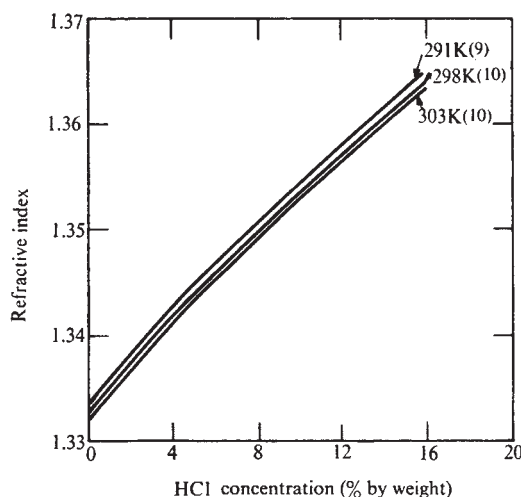


Fig. 1 Temperature and concentration dependence of the D line refractive index of aqueous HCl solutions^{9,10}. The concentration dependence from 10 to 30% by weight HCl is almost perfectly linear. An empirical equation for the temperature dependence is given in the text.

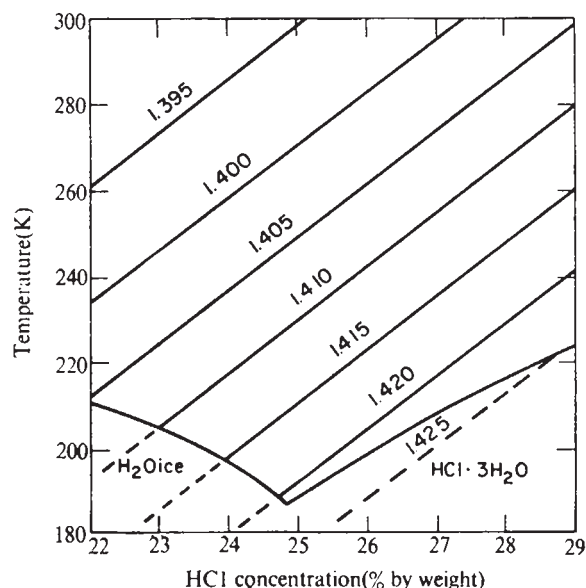


Fig. 2 Refractive index, n_F , at low temperatures. Density data of Garrett *et al.*¹¹ were used in conjunction with the Lorentz-Lorenz equation to calculate n_F at temperatures for which no direct measurements exist. The freezing lines for precipitation of H_2O ice and solid $HCl \cdot 3H_2O$ are indicated. Refractive index isopleths drawn in below the equilibrium freezing lines refer to supercooled solution, not to the solids.

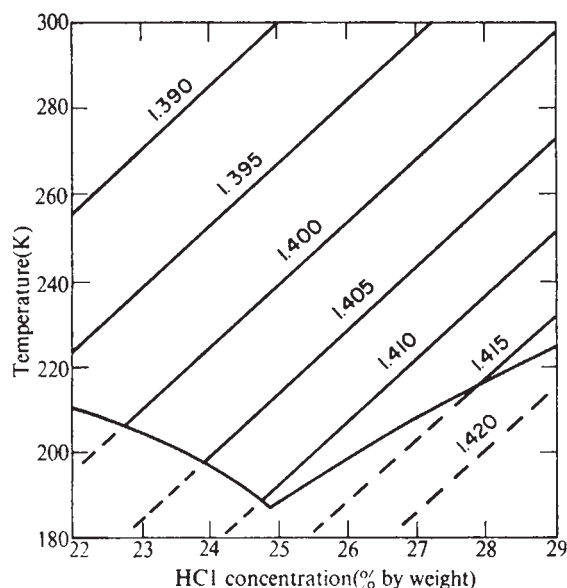


Fig. 3 Refractive index n_D at low temperatures (see legend to Fig. 2).

Values of n_F and n_D calculated by means of the Lorentz-Lorenz equation for HCl solutions between 22 and 29% by weight HCl and from 180 to 300 K are shown in Figs. 2 and 3, respectively. The equilibrium freezing lines are drawn in for reference.

A refractive index of $n_F \geq 1.42$ corresponds to the cloud formation conditions given by Lewis⁷. Indeed, the most crucial questions affecting the plausibility of such clouds on Venus concern the true thermal structure of the atmosphere of Venus near the 50 mbar level. Lewis^{6,7} has stressed the extremes necessary to condense water, in any form, in the Venus clouds. I think that rejection of HCl solution clouds because of the discrepancy between the ~ 1.420 to 1.428 values which I have calculated, and the 1.45 ± 0.02 found by Hansen, is extremely unwise. Although I would not assign a high *a priori* probability to the existence of such an HCl solution haze layer on Venus, I consider this more probable than the

alternative, C_3O_2 . Carbon suboxide has a vapour pressure so high that it exceeds, by at least a factor of 10^3 , the spectroscopic upper limit on gaseous C_3O_2 . Furthermore, there is a high probability that the refractive index of C_3O_2 at < 240 K is > 1.49 .

Although conclusive identification of the chemical constituents of Venus's clouds is not yet possible, the accumulating evidence eliminates most of the proposed alternatives. Perhaps the most interesting conclusion will concern the presence or absence of pure liquid water and H_2O ice clouds, but current evidence is against the possibility of their existence on Venus^{1-3,6,7,12}.

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- ¹ Arking, A., and Potter, J., *J. Atmos. Sci.*, **25**, 617 (1968).
- ² Coffeen, D. L., *Astron. J.*, **74**, 446 (1969).
- ³ Coffeen, D. L., and Gehrels, T., *Astron. J.*, **74**, 433 (1969).
- ⁴ McElroy, M. B., *J. Geophys. Res.*, **73**, 1513 (1968).
- ⁵ Harteck, P., Reeves, R. R., and Thompson, B. A., *NASA TN D-1984* (1963).
- ⁶ Lewis, J. S., *Astrophys. J.*, **152**, L79 (1968).
- ⁷ Lewis, J. S., *Icarus*, **11**, 367 (1969).
- ⁸ Diels, O., and Blumberg, P., *Chem. Ber.*, **41**, 82 (1908).
- ⁹ Schreiner, E., *Z. Phys. Chem.*, **133**, 420 (1928).
- ¹⁰ Eisey, H. M., and Lynn, G. L., *J. Phys. Chem.*, **27**, 342 (1923).
- ¹¹ Garrett, A. B., and Woodruff, S. A., *J. Phys. Colloid Chem.*, **55**, 477 (1951).
- ¹² Potter, J. F., *J. Atmos. Sci.*, **26**, 511 (1969).

Sea Tidally Induced Variations of the Earth's Magnetic Field (Leakage of Current from the Atlantic)

INTEREST has recently revived in the possible effects of the sea tides on the Earth's magnetic field; modern instrumentation has made direct measurement possible^{1,2}. Malin³ has shown how the semi-diurnal lunar variation arising from the ocean and sea tides may be inferred from the complete lunar variation observed at any given station. The time seems appropriate to begin work on detailed theoretical models taking into account the topography of the coastline. Here we continue work^{4,5} specifically aimed at understanding the magnitudes and distributions of ocean and sea tidally induced magnetic fields over the British Isles.

As the induced magnetic fields are very small, their effects on the motion of the sea water can be ignored. It follows that we can separately consider effects arising from oceanic tides and shallow sea tides; the results obtained can be freely added. In this communication we shall restrict our attention to the effects of tidally induced electric currents leaking from the Atlantic Ocean into the shallow seas around the British Isles as suggested by Bullard⁶. An order of magnitude estimate of the magnetic field arising is 0.15γ , except near the coast. Pending full calculations of the Atlantic Ocean electric currents caused by tides we suppose that these are unknown, but that their influence at the edge of the continental shelf is uniform. The sea is taken to be of uniform depth (~ 80 m) which is much less than the ocean depth (~ 4 km). The sea-ocean interface is supposed to lie along the latitudes 48° N and 62° N and the longitude 13.34° W. These constitute the lower, upper and the left hand boundaries respectively in Figs. 1 and 2. The land and the right hand boundary (8.375° E) are taken to be insulators. The sphericity of the Earth is ignored. The dynamo

Continued on page 317

REVIEW SUPPLEMENT

Wheels Still in Spin

THE plight of the university libraries today commands special attention. Faced on one side by an unprecedented rate of increase in the cost of books and journals, and shadowed on the other by the certainty of ever increasing student numbers as the expansion of higher education in Britain begins to take effect, librarians are nevertheless expected to balance a budget which takes both of these factors into consideration, yet using funds which have a lower rate of growth than either book prices or student numbers. To balance the equation is clearly impossible, and librarians are left only with the decision of where best to make economies to least hamper the efficiency of the service they try to provide.

But this is only part of the problem. University libraries constitute a significant proportion of the publishers' market, especially in the fields of science and technology. What will be the effect on these publishers of a cut back in the rate of library acquisitions? Will this mean further and inescapable price rises? Is there, in fact, a solution to the vicious inflationary spiral of rising costs and decreasing sales?

Much of the blame must be attributed to the present economic climate. This has caused large increases in the cost of paper and other printing materials. But printing charges have risen disproportionately to these costs and this seems to be due chiefly to the long overdue efforts which printers are now making to drag themselves into the 20th century. The purchase of modern, high-speed printing equipment, the use of computer typesetting techniques, all involve immense expenditure which takes years to recoup. And of course specialized textbooks and monographs which are printed in limited editions in short production runs do not make the best use of these sophisticated facilities. It comes as no surprise, therefore, to learn that a printer estimating the cost of printing a monograph now should expect that his price will have risen by 20 per cent in twelve months' time.

Publishers, too, have had a hand in increasing prices, in particular as a consequence of the adoption of much more sophisticated financial techniques. Discounted cash flow techniques, for example, where the need to recover the initial outlay as rapidly as possible means that the break-even point must be reached much more quickly, can add remarkably to the price of a book. And the libraries do not escape scot free, for the widespread use of photocopying can make savage inroads into a publisher's profit; and this seems to apply not only to those countries where copyright is ignored.

All of this goes some way towards explaining why the average price of journals of British origin has increased by 11.2 per cent since 1969 and by over 50 per cent since 1965. Comparable figures for scientific and technological books show a 10.1 per cent rise in the average price since 1969 and a 38 per cent increase between 1966 and 1970 (*Lib. Assoc. Rec.*, 72, 289; 1970). And this in turn explains why the librarian in one of the newer universities spent £6,000 less than he would have liked to on books in 1970, and why publishers' representatives no longer visit the library of one of the older universities, knowing

full well that it can acquire no more stock until the next fund allocation.

What will the librarians do? The immediate step is, of course, to cut down on acquisitions, and it seems, perhaps surprisingly, that they are maintaining stocks of journals at the expense of books. The principal areas where economies seem to hurt least are the expensive reprints which are so much in vogue at present, and research monographs. In other words the resources for undergraduate learning are being maintained at the expense of the resources for research.

It seems, furthermore, that much greater use will be made of the inter-library loan service. Writing in the first issue of the *NLL Review*, a new quarterly designed to disseminate information about the activities of the National Lending Library for Science and Technology at Boston Spa, the director, D. J. Urquhart, notes that only a few special libraries in Britain take the inter-library service into account when planning acquisitions, and that the future growth of the NLL will depend to a great extent on how this attitude changes in the next few years. In terms of staff and finance, however, he is sure that present NLL procedures are capable of handling several million requests each year; in 1969, loan requests totalled only 928,300 (*NLL Review*, 1, 14; 1971). It seems possible that the library could indeed handle the vast increase in requests which increased use of this service by the universities must entail.

And how will the publishers respond to this threat to their sales and profits? At present, the future seems gloomy and uncertain. Some publishers have already noted decreasing sales since 1965, whereas others have yet to feel the pinch. There are indications that prices may stabilize somewhat in future as the full benefits of the new printing techniques are felt, but this is only part of the answer. What does seem certain is that Britain is declining in importance as a world market compared with Europe. A publisher who five years ago was selling 60 per cent of his stock in Britain finds today that this figure has been cut by half. The reason for this is the exorbitantly high cost of printing science texts in a language other than English. The tiny print run involved in such publications compared with English language editions means that a scientific book selling for £10 in Britain is really very cheap in Europe.

Perhaps justifiably, however, publishers seem to feel that if the quality of the book they publish is high, then sales will be forthcoming. It should come as no surprise, therefore, to those charged with disbursing financial support to the universities, if librarians soon ask for a larger share of the vote; in the end it will be the students who will suffer most if their resources for learning are weakened.

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General Books

Events at Los Alamos

A Random State. By Thomas McMahon. Pp. 246. (Macmillan: London, 1970.) £1.90.

A Random State is an extraordinary first novel for its author to have written. He is a young Harvard post-doctoral bio-engineer born in Ohio in 1943: his novel is about life among the scientists and engineers on the mesa at Los Alamos between 1943 and 1945—and it makes one feel he was there. . . . If that isn't the first criterion by which the satisfactoriness of such a novel should be judged, and one of the criteria by which an author's talent is shown up, I'd like to know what is. It seems to me a tall order for Mr McMahon to have taken on, to write a first novel about what corresponds to a group of real people, most of them still alive, in a period of their history, already well documented, when he himself was only just born: and to write not a history but a novel in which his naturally synoptic view of individual human beings means he must integrate the inward looking side of their existence with the outward. Added to that he has to introduce some science into the text. And meanwhile he is learning to write! The order is tall, and Mr McMahon's degree of success in filling it varies from one item to another; but his originality and talent are not in doubt.

Mr McMahon has coped with the first item astonishingly well. Names are changed and natures are changed just so far as to deter the reader, very properly, from treating the book as a *roman à clef*. For example, Seth Neddermeyer, the name of the theoretician who first suggested implosive detonation for The Bomb, is changed to Nelse Nachtigall: Neddermeyer was introverted, reserved and shy; Nachtigall is loud and libidinous. Oppenheimer's first response to Neddermeyer's suggestion was opposition, though he had earlier thought about collapsing suns in this context: Sandemann in the novel says "This is a beautiful thing you've done for us". Yet Mr McMahon's scene works.

Although *A Random State* is short—and sometimes a little non-linear, as if it had been inexpertly cut—the story is complex. It is focused on a thermodynamicist called Harold MacLaurin, and is told in the first person by MacLaurin's son, Timmy, looking back in the year 1960 on events beginning in 1942, when his father was thirty-two and he himself was twelve. MacLaurin is going through an unexciting spell at

MIT, is drawn into the Manhattan District project, works for a few months at Oak Ridge and then goes out to Los Alamos. His inward story is of his marriage breaking up in Boston, a new love beginning in Oak Ridge for a young secretary, Maryann, and that love coming to grief on the mesa. His wife stays behind in Boston, but Timmy goes with him, staying by his side throughout. (Staying as close by his side as only a first-person narrator stays by a central character's. From time to time one asks: how in God's name did a thirteen year old boy get there?)

In retrospect the inward story seems to dominate the shape of the novel, the effect being enhanced by Timmy's experience of being initiated vaguely into love and sexual life by Maryann. Yet it is MacLaurin's outward story that comprises the original, fascinating part of the book. McMahon has coped with this item by giving us a boy's-eye view of it; but it is a boy's-eye view interpreted fifteen years later by a man who has himself by then become a physicist. Mr McMahon has hit that off with beautiful accuracy. One after another different glimpses of the scientists float by, instantly recognizable as true: of their anarchic unbiddableness, exacerbated by the FBI's efforts to bid them; of their round-the-clock devotion to the work in hand; of their exuberant, uncomplicated randiness—anyone who doesn't realize that within the ivory tower of science there's a lot of hearty you-know-what going on is in for a surprise; of their simple, sarcastic humour; of their dark nights of the soul about the value of their work—somewhat self-conscious dark nights, if one may say so. But what above all gives McMahon's view the luminous glow of *verismo* is his gift for visual evocation—whether he was actually there to see what he evokes or not. The clots of red, Tennessee mud on the shoes of visitors coming back from Oak Ridge—the FBI made them wash it off: the blue spot of dye on Maryann's finger, "resembling an opal held by an invisible band": Dr Orr (Bohr!), whitehaired, seen through a mirage uprooting a flowering plant from the desert sand. . . .

With some of the other items on his tall order Mr McMahon is less successful. The inward looking side of his story is less original, even less interesting, than the outward. The narrator is recovering from a nervous breakdown and Maryann has identity troubles—two clichés which could now well be allowed to rest in peace. In fact, Maryann may be

acceptable as a fantasy in Timmy's slightly maundering erotic imagination, but as a corporeal presence on the mesa she doesn't begin to exist beside Nelse, Richi and Co. As for her being able to influence anybody on the mesa apart from the rather wet Timmy, I can only say Moonshine!

Coming lastly to the specifically literary items: when McMahon introduces some hydrodynamics into the text, it rather sticks out, unassimilated. The switch of the narration from being addressed to the reader to being addressed to Maryann must seem very mysterious to any reader who doesn't realize it is Mr McMahon, not the narrator, who is trying to solve a literary problem thereby. And his American editor must have been asleep, to miss Timmy's mother's family having a farming background on p. 8 and her father being sacked on p. 19 after fifteen years in a factory, and to permit grammatical *bêtises* such as "How unfortunate it is that one always has to have before him the apprehension that his readers won't believe him". But Mr McMahon should not worry: he can choose his words with the best of them. "Richard Mundi, B.S., M.Sc., Ph.D., physicist, mathematician, prankster, bongo player. Harold MacLaurin, Ph.D., physicist, fluid mechanist, potential scientific clairvoyant, adulterer." Let any novelist take the same number of words for the same purpose and beat that!

WILLIAM COOPER

Cold Comfort

Vitamin C and the Common Cold. By Linus Pauling. Pp. 122. (Freeman: San Francisco and Reading, March 1970.) £1.90 boards; £0.80 paper.

LINUS PAULING is well known for his contributions to chemistry and to the cause of peace, and his scientific knowledge and his concern for human welfare can both be seen in this short book. The book is intended for the general public, although it includes some quite detailed quotations of scientific results and a list of references in an appendix.

The introduction is anecdotal and describes how the author and his wife noted a marked reduction in the number and severity of common colds after they began taking regular large doses of ascorbic acid. There follows a straightforward chapter on common colds and several on ascorbic acid; the latter give simple accounts of the discovery of the vitamin and its effects. There is an

interesting chapter on "Vitamin C and Evolution" which points out that most species can synthesize relatively far more ascorbic acid than man ingests. Pauling postulates that there may be an advantage to the organism to shed the work of synthesizing an essential substance provided that it is readily available in the food supply. He claims that this has been proved for micro-organisms, but it seems to remain a hypothesis for higher organisms, although it gives him an opportunity to point out that gorillas eat 4.5 g of ascorbic acid a day and that if man lived entirely on a mixture of raw plant foods he might consume almost as much.

Pauling attacks the US and British standards for daily intake of ascorbic acid, although they represent much more vitamin than is required to cure the average case of scurvy. He points out the biochemical individuality of man and suggests that an optimum intake for some people may be as high as 10 g per day. In attempting to buy large amounts of vitamin C for himself he has discovered that this may be bought quite cheaply in 1 kg jars or as multivitamin tablets; however, it costs several times as much under a trade name or on prescription and up to eight times as much in the form of health products. One is bound to have sympathy with this attempt to protect the general public from exploitation, although a good mixed diet is probably all that is necessary to preserve the vitamin intake of all but a few special categories of the population.

The whole object of the book, however, is to persuade the reader that taking large doses of ascorbic acid will prevent and ameliorate colds and this is alluded to repeatedly. He presents the results of field studies such as that of Glazebrook and Thompson which showed that those on a vitamin C deficient diet had rather more colds and substantially more bacterial infection, particularly pneumonia and rheumatic fever, than those in the same group who had vitamin C supplements; he also reports other small studies suggesting that vitamin C may reduce the duration and slightly reduce the incidence of colds. Then (p. 45) he says that "... the common cold can be almost completely controlled by use of still larger amounts of ascorbic acid, several grams per day". From then on no controlled or convincing studies are presented—but chiefly claims that colds are "aborted" by the very early use of large doses of vitamin C; this contains the obvious fallacy that the more often one treats very mild symptoms, a scratchy throat, perhaps, the more often will one treat something which was never going to be a cold anyway and the more often will this be regarded as "aborting" the cold.

The only safeguard is to use a double-blind placebo-controlled trial. In an appendix are found reports of several other studies, most of which did not confirm an effect of oral ascorbic acid on colds. These are criticized on the grounds that too little ascorbic acid was used, for example 200 mg per day, or that treatment was deferred until after the cold had begun, although on p. 47 he quotes with approval such a treatment recommended by a Dr Régner. He likewise discounts a volunteer study performed at the Common Cold Unit, Salisbury, Wiltshire, although 3 g/day was given before virus infection, because the population was probably ascorbic acid deficient—but this would have increased the difference due to a vitamin supplement. He also criticizes it because the numbers used would have only detected a decrease of 40 per cent with a statistical probability of 5 per cent—but this was the sort of claim which it was desired to test, and effects on the duration of disease and the excretion of virus would have been more easily detected and were not. Nowhere in the book are there reports of data from research, which the author says he is carrying out (p. 90), which prove objectively that ascorbic acid prevents colds. One comes sadly to the conclusion that Pauling is so convinced of the efficacy of vitamin C in common colds that he is no longer capable of regarding the problem objectively and realizing that there is little or no evidence that it does any good. While I agree that drugs are given quite unnecessarily and possibly with harmful effects in this disease, that is no justification for presenting to the general public, in a form which may look to them like scientific proof, propaganda for a harmless but no more effective remedy. DAVID TYRRELL

Megalithic Lunations

Megalithic Lunar Observatories. By A. Thom. Pp. 127. (Clarendon: Oxford; Oxford University: London, January 1971.) £3.00.

In 1962, Professor Alexander Thom startled the archaeological world by suggesting that the megalithic stone circles of Britain, dating from around 1800 BC, were laid out with the use of a consistent unit of measure, the megalithic yard. The notion that a standard unit of length—precisely 2.72 feet—should have been used throughout Britain, from Wiltshire to the Hebrides, at so early a time did not tally, and indeed still does not tally, with what we know of social organization in Europe in later Neolithic and early Bronze Age times. Some archaeologists tended to dismiss the notion as untenable, as yet another attempt—and there have been many—to read into prehistoric monu-

ments (especially those of megalithic construction) patterns and explanations which are simply figments of the observer's imagination.

These unworthy thoughts do scant justice, however, to Thom's surveying skill (he was Professor of Engineering Science at Oxford until 1961) and to the lucidity of his presentation. A frequency distribution for the radii of the British stone circles, given in his *Megalithic Sites in Britain* (Clarendon Press, 1967), shows peaks at regular intervals. The obvious hypothesis is that these peaks represent multiples of a unit of length, and Thom presented statistical tests for the determination of this unit and for the significance of its emergence from the data. He presented a similar frequency distribution for declinations defined by the alignments of pairs of stones in megalithic constructions, and suggested very plausible correlations with the declinations of the midsummer and midwinter Sun, and with the extremes of the Moon's rather more complicated movements—the major and minor standstills—throughout the months and years.

Megalithic Lunar Observatories takes this work a stage further by looking much more closely at the Moon's movements in the sky as viewed from north Britain, and of prehistoric man's efforts to follow them, understand them and predict them.

It should be said at once that Thom's book is written in the same spirit of sober yet optimistic enquiry as his earlier writings. Indeed it is imperative that archaeologists and other critics, who may not be entirely persuaded by Thom's findings, should distinguish between his approach, with its meticulous fieldwork and detailed presentation of evidence, and the megalithic lunacy of some of our contemporaries. To the layman Thom's carefully argued claims may seem as disconcerting as the fanciful speculations and conjectures of Professor Lyle Borst which appeared in *Nature*, 224, 335; 1969. Readers may remember that Borst claimed to find units of length and geometrical practices, of the kind seen already in megalithic constructions, in the plans of certain mediaeval cathedrals. On this basis alone, without the support of any archaeological evidence whatsoever, Borst suggested that these cathedrals were built on, and aligned on, megalithic constructions supposedly once occupying the same location, and of which not a trace remains today. Such a claim as Borst's, unsupported by any detailed presentation of the hard observational evidence on which it might be based, is outlandish: I feel strongly that it should never have been given the recognition which publication in *Nature* implies. In complete contrast to these deplorable fantasies (and in



Stonehenge, one of the most prominent examples of what Professor Thom believes may be megalithic lunar observatories. This cut, first published in William Camden's *Britannia* (1695), is taken from *Aspects of Prehistory* by Grahame Clark (University of California: Berkeley, Los Angeles and London, 1970, \$5.95).

possession solely of the scanty data which Borst has so far published one can call them nothing else), Thom's assertions are respectably documented. He reports his data as well as his deductions and hence offers the opportunity—as a scientist should—to check his conclusions against his observations.

Thom's thesis is breathtakingly ambitious. He believes that the inhabitants of prehistoric Britain around 1800 BC were not simply observing the monthly progress of the Moon from north to south and back again, together with the further cyclical shift in the northern and southern extremes of these lunations, with a period of 18.61 years. These are of the order of 23 degrees and 5 degrees respectively. They had detected, he feels, a further perturbation in the Moon's orbit of nine minutes of arc. This is greatest when the Earth, Sun and Moon are all in the same plane, so that gravitational effects are maximal and—what may particularly have interested the prehistoric Moon watchers—eclipses are to be expected. This perturbation was first discovered (or now, perhaps, re-discovered) in the sixteenth century by Tycho Brahe and apparently escaped the notice of the astronomers of the Classical world. The claim implies, then, that the prehistoric inhabitants of Britain were observing more accurately and successfully than Ptolemy of Alexandria, whom they preceded by two millennia!

To obtain lines of sight of sufficient accuracy it was not good enough to use a pair of standing stones, unless these were very far apart. Thom shows that a single stone could, however, be used very accurately in conjunction with a

prominent and remote foresight—a mountain peak, a narrow valley, a small island—on the horizon. He has conducted accurate surveys at a number of Scottish megalithic sites. He finds that at several of them one or more standing stones do in fact line up sufficiently closely with prominent natural horizon features to define a line whose declination is precisely that of the rising or setting Moon at its major standstill, to within a minute or two of arc.

In assessing these claims we have to admit that some of the alignments—standing stones arranged in a straight line—do indeed give a line, quite independently of natural horizon features, which indicates, within a degree or so, one of the lunar standstills. Thom therefore seems justified in regarding such sites as lunar observatories because the fortuitous arrangement of the stones along just these lines of sight is perhaps unlikely (although with rising and setting positions for each of four lunar standstills, not so unlikely as at first appears).

The next step in Thom's reasoning is more problematical. He examines the stretch of horizon intersected by the path of the setting Moon at one or other of its standstills, in order to spot natural features which might have served as a foresight. The azimuth of such a natural notch or prominence as viewed from each standing stone of the monument is then measured, and the declination calculated. When there are several stones extending along a line perpendicular to the line of sight, the declination, as determined by the foresight and each stone in turn, differs by a few minutes of arc from that obtained

using its neighbours. In some cases the difference in declination is of the order of nine minutes—the magnitude of the perturbation. Both refraction by the atmosphere and parallax effects are considered, and the size of the Moon's disk (semidiameter 15 minutes) so that Thom plausibly demonstrates that sites such as Temple Wood or Ballymeanach in Argyllshire could have been used in this way to detect the perturbation.

But were they? This is more difficult to demonstrate. A frequency histogram is indeed presented of the declinations chosen for accurate survey. But for some of these, the only prehistoric feature is a single menhir (standing stone). The foresight on the horizon has to be selected by the observer. Naturally, if we are studying lunations we look to see if there are natural landmarks in the approximate directions of the lunar standstills. But supposing we find them (as Thom has done), there is no obvious way of testing whether this merely is fortunate coincidence or the meaningful result of careful siting of the menhir in prehistoric times. That the modern observer has to select the foresight—which is the case for all the accurate measurements when we are dealing with small perturbations to be measured in minutes of arc—brings a risk of circular argument. This risk is not present when pairs or alignments of stones, erected in prehistoric times, are being used to give lines of sight: but these cannot be used so accurately as to detect such relatively minor perturbations.

I am left, therefore, greatly impressed with Thom's marshalling of the evidence which he has gathered through accurate field survey, but uncertain as to how it may critically be assessed. His former work on the megalithic yard, and on solar declinations, gave very suggestive frequency distributions. These were based entirely on measurements of the positions of stones erected in prehistoric times. So far no one has come forward with a satisfactory explanation for these apparent regularities other than that propounded by Thom himself: that they are the result of the deliberate intention, and the mathematical and astronomical awareness, of those erecting them. In the present case, however, the selection of the foresight by the modern observer makes the demonstration less compelling. Thom might well answer that the horizon features which he has selected are in each case obviously the most appropriate, and that the number of suitable horizon features is in any case small. Such an argument would ideally lead to some test of significance for these data, and this is now desirable. Thom has shown how these monuments could be used to observe the Moon's behaviour with an accuracy not again

equalled until the time of Tycho Brahe, but not necessarily that they were so used.

Troubled by these doubts I do not yet feel able to follow Thom in his interpretation of the grid-like stone rows of Caithness. He argues that they were used not as sight-lines but as computers for extrapolation of the observations of the monthly standstills of the Moon, to give a more accurate value for the extreme position at the 18.6 yearly major standstill than could be observed directly. Again we are speaking here in terms of minutes of arc, not degrees, and the interpretation depends on the demonstration that such accuracy of observation was in fact achieved in prehistoric times.

The exposition in the book is both lucid and compelling, although fairly tough going for anyone, such as myself, who has to learn his astronomy as well as his megalithic metrology from Thom. It is an exciting book, enlarging, like its predecessor, our view both of what prehistoric man may have achieved and how we may know of it. Prehistoric archaeology is fortunate to have in Thom so skilled and forceful an advocate for "megalithic" man. The evaluation and the interpretation of these results is now a challenging task for the prehistorian.

COLIN RENFREW

Medieval Optics

John Pecham and the Science of Optics—Perspectiva communis. Edited with an introduction, English translation and critical notes by David C. Lindberg. (The University of Wisconsin Publications in Medieval Science.) Pp. xvii + 300. (The University of Wisconsin: Madison, Milwaukee and London, November 1970.) \$15.

IN recent years our understanding of medieval science has been greatly enhanced by the "University of Wisconsin Publications in Medieval Science" under the general editorship of Professor M. Clagett. Professor Lindberg's volume, the fourteenth in the series, is a particularly welcome addition, dealing as it does with medieval optics, a subject which has suffered from comparative neglect. John Pecham's *Perspectiva communis* was a particularly influential short textbook on optics, and Lindberg lists sixty-two extant manuscripts and eleven early editions. The author was a Franciscan friar and, from 1279 until his death in 1292, Archbishop of Canterbury. Lindberg thinks that most probably he composed the *Perspectiva communis* in the period 1277–79 when he was teaching at the Papal University in Viterbo and Rome. After the first composition, which Pecham says that he did not intend for publication, he wrote a new recension, which paradoxically

is only fully extant in one manuscript.

In his carefully established edition, Lindberg makes both versions accessible to us, although this only occasionally requires him to give parallel texts. His English translation makes the work much more approachable by the modern reader, but he admits that he has translated less literally than he would have if he were not providing the Latin text. This occasionally leads him into interpreting Pecham's thought in ways that may not be universally acceptable.

Lindberg finds that Pecham's treatise is based principally on the *Optics* of Ibn al-Haitham (known in the West as Alhazen) who flourished in Egypt in the late tenth and early eleventh centuries. Lindberg also sees the influence of various other writers including Robert Grosseteste, Roger Bacon and probably Witelo. In the case of Pecham's relation to such authors I rather missed the more extended critical discussion of the type given in recent editions in this series by E. Grant and M. Clagett. I would also like to have seen rather more of the obscurer passages of the treatise subjected to such discussion, and a feeling of curiosity remains about the *Tractatus de perspectiva*, which Lindberg regards as being an earlier work of Pecham's, but which is barely mentioned in his introduction. I hope that financial stringencies have not limited this volume undesirably. Nevertheless we are provided with copious references by means of which such problems may be more easily investigated.

The *Perspectiva communis* is divided into three parts in accord with the ancient division into optics, catoptrics and dioptrics. In the first part Pecham accepts a basically intromission theory of vision but still holds that the "natural light of the eye" is necessary to make the incident rays proportionate to the visual power. He adheres chiefly to the rectilinear propagation of light, but somewhat complicates the situation by at times speaking of a natural tendency of light to roundness and of a secondary diffusion outside the main beam. The modern reader will also realize that more mathematical precision could have been obtained if Pecham had not so often spoken in terms of pyramids of light rather than rays. When considering reflexion in the second part of the work Pecham of course recognized the equality of the angles of incidence and reflexion, but his treatment of refraction both in the first and in the third part is so bound up with his idea of its cause that only a qualitative account is given.

Our own education often disguises from us the difficulties that early writers had to face. It is not the least of the values of editions such as this that they bring these difficulties forcefully to our notice.

A. G. MOLLAND

Spells, Bound and Broken

Religion and the Decline of Magic: Studies in Popular Beliefs in Sixteenth and Seventeenth Century England. By Keith Thomas. Pp. xviii + 716. (Weidenfeld and Nicolson: London, January 1971.) £8.00.

HISTORIANS have been so impressed by the innovatory nature of modern science that there has been a tendency to ignore or undervalue the systems of natural philosophy which were discarded during the triumphant march of progress. Neoplatonism, Aristotelianism and the magical systems have been regarded as stumbling blocks, inhibiting the scientific enlightenment. This has inevitably generated a view of the great scientific pioneers as men standing apart from their cultural environment, in which authoritarian and traditional beliefs persisted tenaciously among the educated classes and persisted most among the uneducated.

Recent researches have produced a strong impression that our simple established estimates of scientific progress are seriously deficient. The scientists of the seventeenth century from Gilbert to Newton, hitherto celebrated for their workmanlike independence and lack of susceptibility to metaphysical speculation, have proved to have strong intellectual affinities with formally discredited intellectual movements. We are now forced to consider whether the animism of Gilbert, Boyle's interest in witchcraft, or Newton's alchemy represent intellectual aberrations or integral parts of their natural philosophy.

Resolution of this issue has been handicapped by lack of serious historical studies of the non-mechanistic world views. The present book by K. V. Thomas repairs this neglect in a brilliant, superbly documented and comprehensive study of magic, witchcraft and astrology in England during the sixteenth and seventeenth centuries. This period is sufficiently broad to exhibit the genesis and unfolding of experimental science and the spectacular decline of magic during the final decades. An important contributive factor to the success of this book is the author's controlled and sensitive use of the methods and materials of social anthropology. Like the best writings on the latter subject, this book gives a sound balance between the analysis of data and general assessments. With scientific thoroughness the author depicts the problems and goals of Tudor and Stuart communities, magic being relevant to this situation at many points. Throughout the book, I was impressed by the relevance of this background to the understanding of the outlook of the pioneers of experimental science.

The interaction between established

of chemistry delivered in Oxford at the turn of the century, long before the history of science had taken on the modern forms either of scholarly study or of popular exposition. They are all accessible elsewhere, and several of them are already familiar. They are all easy reading for the newcomer to the history of chemistry, although two would be hard going, if not unintelligible, to a non-chemist.

Apart from the final one on the College laboratories at Oxford, each chapter is biographical. Seven chemists, Priestley, Lavoisier, Dalton, Davy, Berzelius, Faraday and Cannizzaro, are treated as heroes, each placed in a lime-light of approbation which leaves the rest of the chemical stage rather dim. The Priestley chapter makes a tame start, little more than a recital of discoveries, but Lavoisier gets more animated treatment, and justice is done to his skill as administrator and man of public affairs. The Dalton essay was prepared in time for the bicentenary in 1966 and therefore failed to benefit from the new studies published between 1966 and 1968. All the same, this is in many ways the best chapter for a reason which Hartley generously acknowledges, his debt to the then recent work of a much younger historian of chemistry, Dr Arnold Thackeray. There is a very good treatment of those non-scientific traits in Davy's character which affected his scientific methods. Paradoxically, the heroic treatment serves Davy better than it does Berzelius, the real reasons for whose great stature are never quite shown. To see Berzelius whole, one must see chemistry whole, and in this essay Hartley keeps to a narrative manner which falls short of historical assessment. The essay on Faraday as a physical chemist is successful because it limits itself to a part of his work and outlook which is compact enough for all its principal features to be put into the volume of a short lecture.

The Karlsruhe conference of 1866 attracted a lot of centenary examination. Hartley's essay is a good account of the events, but, being associated with the name of Cannizzaro, reaches the conventional conclusion that its principal benefit was the *hor-concours* distribution of Cannizzaro's *Sunto*. There was more to this famous meeting than this, as Mendeléeff himself realized. The Armstrong essay is particularly useful in its estimates of Gerhardt and (particularly) Kolbe, but its greatest value will be for some future historian looking for personal reminiscences. For the historian of chemistry today, however, the most useful chapter of all is the one carrying the earliest date (1931), on "Faraday's successors and the theory of electrolytic dissociation". We have barely begun to write the history of chemistry in the 20th century, and we

shall need to learn how to elucidate and then how to expound the evolution of chemical theories of depth and complexity. It is easy to write about the early history of electrolytic theory. The post-Faraday era is far more difficult, even at the level merely of history-of-ideas. This is as far as Hartley could take it, and he had obviously to consider very carefully how to narrate the succession of theories associated with Arrhenius, Kohlrausch, Bjerrum, Onsager, Debye and Hückel.

The result shows how the writer of the history of chemistry ought to control his material, with technical accuracy over the detail of substances and effects going hand in hand with a sense of the movement of ideas. It is claimed that the book will be useful in universities and sixth forms. But no serious student will be helped to discipline himself by a book the documentation of which varies, chapter by chapter, from moderate to non-existent. No one writing biographical articles on these subjects now would dare ignore the considerable work that has been done on most of them in recent years. The OUP editors must take the blame for this. It would not have been difficult to find someone to prepare notes on further reading, which would not have interfered with Hartley's own texts, but would have made this into something like a modern book. But it is always clear, never tedious, and if put into the hands of the right sixth formers might inspire some of them to master the art and craft of history. We might then be ready in a generation to write that history of modern chemistry which, in spite of many attempts, has not yet appeared. The best parts of this book show how it might yet be done.

FRANK GREENAWAY

Man and the Heart

The Battle Against Heart Disease: A Physician Traces the History of Man's Achievements in this Field for the General Reader. By P. L. Baldry. Pp. 189. (Cambridge University: London, March 1971.) £3.00; \$10.

To compile a book on the cardiovascular system and its diseases demands wide knowledge and years of practical experience. To write a history of the same subject, however, needs no specific training, for it is necessary only to read a few books and then to regurgitate the information gleaned in a pleasant and readable form. Or so it is thought! In point of fact, both tasks demand an equal measure of understanding and expert opinion.

That this is not always recognized is evidenced by the large number of popular books on the history of medicine, and the general reader is now provided

with yet another "History of Man's Achievements" in the field of heart disease. The book begins with the vague notions of classical antiquity and continues up to the present-day "triumphs of surgery", taking into account the form and function of the heart and then the clinical manifestations, diagnosis, and the treatment of its diseases. The author presents his story in a clear and attractive style, explaining each technicality lucidly and, where necessary, providing brief yet excellent accounts of present-day knowledge. The text is well illustrated but there are no references or bibliography.

The familiarity of a writer with medico-historical material can best be assessed by examining the way in which he deals with antiquity. It is a difficult and distant period and it requires considerable historiographical skill to provide even a superficial survey of it. Adequate appreciation of the secondary as well as the primary sources is essential and it is often on this criterion that some authors flounder. In the first three sentences of the chapter of this work there are three errors and the accompanying map has two misspellings. Moreover, to describe Greek biological thought without mentioning the humoral theory is a difficult task, and to read into the Hippocratic Writings a notion of blood circulation (p. 3) is to add fantasy to misinterpretation.

Admittedly the author is not dealing with diseases in this early period and therefore can perhaps be excused for omitting an account of the humoral theory. His lack of reference to it when discussing the latter history of cardiac disorders cannot, however, be condoned. This defect characterizes a general criticism. The book is compiled in the traditional bio-bibliographical manner with reference chiefly to men and books, but with less attention to essential background developments. As the present day is approached the need for this, of course, becomes less and so the later chapters are the best. Those on congenital heart disease and cardiac surgery are excellent and the author's general aim of encouraging a historical approach to modern medicine is highly commendable.

EDWIN CLARKE

History of Isotopes

Radiochemistry and the Discovery of Isotopes. Edited by Alfred Romer. (Classics of Science, Vol. 6.) Pp. xiii + 261. (Dover: New York; Constable: London, March 1971.) £1.75.

THIS book is a companion volume to Professor Romer's *The Discovery of Radioactivity and Transmutation*, which was published as the second volume in the Classics of Science Series. It traces

the development of research in radioactivity from the work of the Curies in the 1890s to the advances made by Soddy and Fajans in the days before the First World War. The book consists of a 57 page historical essay followed by reprints and translations of twenty-six selected articles of the published work of the scientists of that time, interspersed with editorial comment that provides a connexion between the papers.

Romer's essay clearly brings out the frustrations and problems of the early days of research into radioactivity. Concepts that to us in the 1970s are accepted without argument are shown to have developed over several publications and usually a number of years. That the investigation of the properties of radio-nuclides was not altogether straightforward is also brought out, as is the clarity of thinking and farsightedness of those working in the field. In developing the subject, Romer makes reference to the reprinted papers. It is soon apparent, however, that only one half of the papers are to be found in the current book. The others are in the companion volume. This is certainly a drawback, and confirmed my original surprise that a distinction could be made between the discovery of transmutation and discovery of isotopes in such a compilation of the early work on radioactivity. To be fair this is as strong a criticism as can be made. It would be preferable to have all the necessary reprinted articles in the same volume as Romer's fine historical essay. This would probably have meant omitting some of the original papers to keep the book to a manageable size, but this would then have meant that some of the papers would not have been translated into English, and the compilation would have been that much poorer for it.

This book will appeal to both the practising scientist and the layman. Even without recourse to the technical detail, the book is well worth reading for the glimpses that are given of the character of the scientists involved. The excitement felt by the people when on the verge of important discoveries is vividly communicated to the reader. The comradeship of apparent rivals is exemplified by Rutherford's gesture in the summer of 1903. He was on vacation in England at that time from his position as Professor of Physics at McGill University in Montreal. He called in to see Frederick Soddy and Sir William Ramsay at the latter's laboratory in London and found them involved in an experiment designed to demonstrate that helium gas was liberated in the transmutation of radium. Their experiment was on the verge of success but they were frustrated by a shortage of the working substances, radium bromide. As it so happened,

Rutherford was carrying an ample supply of this substance and he promptly turned it over to Ramsay and Soddy. (One wonders what the health physicists of today would have to say about that!) With the amount of material then available, the experiment was a resounding success and clearly demonstrated the existence of helium. It was after this development that Rutherford was able to postulate that the helium was in fact the result of an accumulation of alpha particles following radioactive decay.

There are few people alive today who actually participated in the exciting developments of the era around the turn of the century. The book will nevertheless be read with interest by people who have a personal recollection of the scientists involved, and the younger generation of scientists and students will no doubt benefit from the knowledge that even the legendary names of nuclear physics suffered frustrations and disappointments in the course of their research. For the non-scientist, this will present a fascinating study of how knowledge is acquired from experimental results. For all readers, the insight into the minds of some brilliant people that one obtains both from Professor Romer's essay and from the well set out reprints of their papers is in itself something for which the book is well worth reading. ALUN JONES

Black Death in Britain

A History of Bubonic Plague in the British Isles. By J. F. D. Shrewsbury. Pp. xi+661+4 plates. (Cambridge University: London, February 1970.) £8.00.

THE particular expertise Professor Shrewsbury brings to this question is that of the bacteriologist. The last great work on the history of epidemics was that of Creighton, a masterpiece in its day but superseded in many respects by advances in medical knowledge. Shrewsbury maintains, rightly, that much historical writing since Creighton has displayed ignorance of these advances, or has failed, even when aware of them, to deduce their implications. He shows convincingly how fallacy and error arise when statements are made about bubonic plague in ignorance of its actual behaviour and the mode of its transmission, or when the word "plague" is used loosely of almost any severe epidemic; the result, he suggests, is the attribution to the disease of "a morbid omnipotence it never possessed".

He is right to castigate some antiquarians of the old school for romanticism, but he is less than fair to those economic historians of the past thirty years who have discussed pestilence in

the context of population movement and of economic change. They have not shown the uncritical credulity about mortality that he abhors, and they have recognized that the crowded quarters of the urban poor were afflicted with epidemic disease more often and more severely than the nobleman's household or the remote rural community. They have noticed too that the "stress and panic fear" associated in legend with pestilence, while real enough, have, like the "dislocation of trade" and the "breakdown in law and order", been localized and brief. Indeed, in suggesting that the effects of bubonic plague on the prosperity and size of towns were, if perceptible at all, short-lived, Shrewsbury has arrived by a different route at a conclusion they have independently maintained in recent years.

Shrewsbury sets out what is now generally agreed about the mode of transmission of bubonic plague to man through the bite of the "blocked" rat-flea *X. cheopis*, and the necessity for a prior and coincident epizootic among the local house-rats before an epidemic (as distinct from a few isolated cases) can occur in human population. He argues that the famous outbreak of 1348, although it certainly caused high mortality in overcrowded urban quarters and in the congested heart of the less scattered and less remote of the rural communities, was without doubt responsible for much lower overall mortality figures than has commonly been supposed, precisely because the establishment of new local foci for the disease depended not only on transmission to a locality but also on the existence there of a dense local rat population in close proximity to human beings. He argues also that *P. pestis* "never established a permanent focus in these islands" but remained "an occasional and exotic visitant" from continental Europe and the Levant. The repeated pestilences of the period 1350-1525 present so confused a picture as to prevent clear identification of all their component diseases; the author sees there indications of typhus, smallpox, influenza, but no unmistakable sign of bubonic plague—nowhere the tell-tale combination of maritime invasion and gradual spread to coastal and inland foci with late spring onset and late summer peak. He concludes that England may have been free of plague from 1350 to 1525, but repeatedly visited by it between 1526 and 1665. London (often although not invariably the port of entry) was hit in 1563, 1593, 1603 and 1625. The "Great Plague" of 1665, bad though it was, was perhaps not more severe than some of the outbreaks in the previous century, and proved, in any case, to be almost the last.

In reading this magisterial and valu-

able book one or two queries do occur. Can we be sure that the aetiological picture has remained constant over five hundred years, so that it is safe to extrapolate from slums of Bombay in 1897 to urban—and rural—England in 1348 or 1563 or 1665? Could the virulence of *P. pestis* have fluctuated significantly in that time? Can we definitely rule out of court a significant amount of interhuman transmission of the disease by sufferers from its pneumonic variant? Can *Pulex irritans*, the human flea, under any circumstances act as a vector for bubonic plague as the demographer Helleiner assumed in his contribution to the *Cambridge Economic History of Europe*? It is on the answers to these questions that the reliability of Shrewsbury's strikingly low estimates of the mortality depend, and it is partly on these in turn that depend assessment of the significance of bubonic plague's irruptions into British history. Whatever the answers, however, much of the author's case stands, and the book will be indispensable for all those interested in the economic history of the British Isles, in the demography of pre-industrial societies, and in the history of epidemics. R. G. LEWIS.

Arctic Voyages

A Chronological History of Voyages into the Arctic Regions (1818) Undertaken Chiefly for the Purpose of Discovering a North-East, North-West, or Polar Passage between the Atlantic and Pacific. By John Barrow. A Reprint with a New Introduction by Christopher Lloyd. Pp. x+380+48. (David and Charles: Newton Abbot, February 1971. First published 1818.) £5.25.

JOHN BARROW, Second Secretary of the Admiralty from 1803–45, wrote two chronological histories of Arctic exploration: the first volume, now reprinted, brought the history from early times to 1818; the second volume, *Voyages of Discovery and Research within the Arctic Regions, from the Year 1818 to the Present Time*... (London, 1846), carried it from 1818 to 1845. Except for the scarcity of the first volume, there is no very good reason why it should be reprinted now. Professor Christopher Lloyd's six-page introduction adds nothing to what he had to say about the book in his recent biography, *Mr. Barrow of the Admiralty, a Life of Sir John Barrow* (Collins, 1970), and his comment, "Of course, we must not expect the standard of scholarship of a modern writer..." is amply supported by Barrow's own assertion, "In the compilation of this brief history no pretensions are set up to authorship—the collecting of the materials... employed no great share

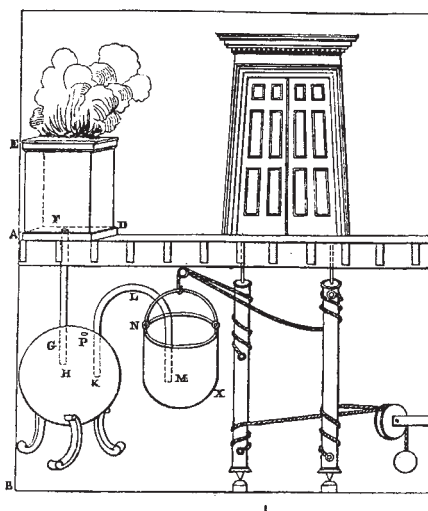
either of the writer's time or research..."

Barrow compiled the first volume of his history to support both his arguments for the existence of a North-West Passage and his determination that the British Navy should find it. He was successful in those aims. In 1818, the Lords of Admiralty sent out two Arctic expeditions and, in the following years, under Barrow's personal supervision, they sent out a series of other expeditions by land and sea, which culminated in Sir John Franklin's tragic voyage, 1845–47, and the prolonged search for it, 1847–59.

If the present volume shows evident haste in preparation, it also shows literary power and shrewd historical judgment—but it is not otherwise particularly useful. The survey is by no means complete, and it contains many errors of fact. In any case, many other works of polar history are now available that are at once more recent, more complete, and more accurate. Barrow's second volume of Arctic history, on the other hand, really would deserve reprinting, for in it he writes with unique authority of the Admiralty expeditions with which he was intimately acquainted. It seems likely that the publishers probably have its reprinting already in view, secure in the thought that anyone who has bought the first volume will surely buy the second.

ALAN COOKE

Open Sesame!



The ghost in the machine exposed. A pneumatic mechanism devised by Hero of Alexandria so that "on lighting a fire, the doors shall open spontaneously, and shut again when the fire is extinguished". Hero lived in the first century AD, but this cut is taken from a facsimile edition of the 1851 Woodcroft edition of *The Pneumatics of Hero of Alexandria* (Macdonald: London, January 1971, £3.00).

Scientism and Humanity

Educational Research in Britain. Edited by H. J. Butcher. Assisted by H. B. Pont. Vol. 1: Pp. 408. Second impression. £2.25 paper. Vol. 2: Pp. xii+298. £3.25 cloth. (London University: London, 1970.)

Educational Research in Britain is a project in which too many different individuals have had a hand: there is little unity of approach and some of the constituents are decidedly sub-standard. The second volume is better than its predecessor in this respect, with five failures, six good passes and four distinctions. The contributions fall into two categories: reports on research institutions (three), and general review articles (twelve, dealing with a variety of themes from the transition between primary and secondary schools to medical education). One of the snags about committing commentaries of both these kinds to cold print, two years after they have been delivered, is that they tend to look embarrassingly out of date. For example, Maureen Woodhall's competent account of the economics of education omits any mention of the current fashion for programme budgeting; and elsewhere some of the investigations singled out as especially promising have produced dull or unintelligible results, while others have produced nothing at all.

This is not to suggest that the whole venture is worthless—as particular contributors demonstrate, institutional reports need not be trivial, and summaries of research need not be ephemeral. M. L. Kellmer Pringle, in her survey of the work of the National Children's Bureau, shows that an organization with a strong sense of purpose and a coherent programme of work can make a significant impact in its first six years of life. And the reviews by Gordon Trasler of research on delinquency and by J. F. Morris of developments in management education, because they develop a clear and persuasive line of argument, shine out against the murky catalogues of miscellaneous bits of research. The latter contain, incidentally, some needless repetitions which could have been removed by a firmer editorial hand.

But the most interesting feature of this exercise is never made explicit—not even in the introduction, which is a disappointingly cursory and formal affair. Both the volumes which have so far appeared mark a conflict of ideologies which needs to be brought out into the open, particularly as it creates internal stress and inconsistency in some contributions. The conflict is between the technological and the humane, and derives from various related sources: first, the fable that educational research can be a science as pure as classical

physics; next, the craving for oversimplification and the search for panaceas; and finally the innumerate's fascination with statistics.

Scientism is a common failing of many of the less secure educationists. Some contributors to Professor Butcher's symposium, underlining the point, show an authoritarian hankering for "control", together with a naive assumption that "controlled" and "scientific" are synonymous. Marion B. Cameron, for example, in her unexciting review of modern language teaching, more than once bemoans "the rarity of any controlled research", even though she admits that it is virtually impossible to match groups of pupils and that laboratory studies are irrelevant to (her phrase) "the classroom situation". In contrast, Jack Wrigley, in an enlightened account of the Schools Council's work, casts doubt on the traditional attempt to eliminate all variables and isolate all factors except the one under study: while Edwin Cox (who makes even religious education sound interesting) goes further by remarking that "the variables of teacher efficiency, class intelligence and environment are too numerous to be controlled". The key point, however, is made by Trasler: the parameters in education cannot be treated as independent of one another, and their relationships are complex rather than simple. Yet a crude experimental model, based on 19th century agricultural research, has held remarkably long sway, yielding nothing except banal generalities and demands for still narrower and more crazily unreal "controlled experiments".

Occam's razor, in unsteady hands, has disfigured the face of education. Morris tellingly condemns "the pre-occupation with extremely simple panaceas", calling in question the notion that "there are universally effective concepts and processes for dealing with individual problems, and that these can be presented in an invariant sequence". But F. W. Land, in discussing the teaching of mathematics, can still pronounce that "it remains a topic for research to determine when best to begin work with the algebra of sets". His article is not alone in implying that students are simply fodder to be uniformly processed through the system, or rejected as waste material if they do not conform to specifications—though John Powell's excellent piece on university teaching methods, and Gerald Williams's sensitive discussion of compensatory education, supply a necessary corrective by stressing individual learners' differences and difficulties, and the need to meet these in more complex, adventurous and imaginative ways.

The worship of the Intelligence Quotient, mercifully a-dying, is still not entirely dead. It has its roots, after all,

far back in primitive mythology. Oedipus, successfully answering the riddle of the Sphinx, was not in this respect so different from the countless succeeding generations who have been subjected to the eleven plus, the Moray House Verbal Reasoning Test, and all too many similar initiation rites devised by the educational underworld to separate heroes from *hoi polloi*. The same mystical assumption prevails: that the life-long fate of any human being can be determined by a brief session with an ingenious conundrum or two. This felony is compounded and lent support by the eighth deadly sin, which is psychometry: the science of fiddling with figures and cooking correlations until they can be made to yield a recognizable mouse or an unrecognizable mish-mash. There are some prime exhibits in the present volume. Thus Cameron, revealing with a flourish that factor analysis has shown "general ability at French" to play a major part in the written and aural comprehension of the French language; and H. P. Pont, on the arts-science dichotomy, convincingly demonstrating that in Thurstone's test of primary mental abilities "the science group derived more from the science half of the test", scoring higher on the scientific and mechanical items. In contrasting style, take Pont again, writing (for the initiated) on more complex themes: "Correlational and factorial analysis showed that, although the divergent thinking tests loaded on the general intellectual factor, the Utility, Consequences and Circles tests also formed an additional factor resembling Sultan's (1962) 'ideational fluency'. Child's was a bipolar factor with physics and geography contrasting with the divergent tests". Mr Pont is a Lecturer in Education at Leeds, and co-editor of the volume under review.

There are, however, heartening indications of a change in perspective—of a search, in Morris's words, "for relevant theories of learning which are not based simply on studies of non-human creatures"—or, one might add, of those who have been dehumanized by what some contributors designate as "systematic research techniques". Time and again the more perceptive writers draw attention to the factors which lie beyond the reach of rudimentary cognitive measurement: parental attitudes, teacher expectations, people's skills in dealing with other people, the creation of a sense of purpose and of personal challenge. There is even one reference to Rosenthal and Jacobson's potent finding that, in human affairs, the researcher's own preconceptions may bias not only what evidence he takes as significant, but also the actual quality of his subjects' performance. When this at last becomes part of the common

currency, and can be taken sensibly into account, educational research may begin to find some valid and legitimate future as a branch of human enquiry.

Meanwhile, Professor Land and his confreres will doubtless stumble on, contemplating their chi-squared tests and bemoaning that the results of new curriculum developments—based on "crude trial and error methods" and without the benefit of "systematic evaluation"—are actually being "made available to the public, without reservations, in commercial bookshops". As Wrigley points out, educational research is at the moment suffering not from a shortage of funds but from a lack of sufficiently outstanding and imaginative practitioners. But unless such research begins to yield better value for money, the sources of financial supply will soon run dry as well: so there is all the greater need to discover less arcane procedures and more relevant paradigms. It must be hoped that Volume 3 in Butcher's series will give a clear signal that this need is being met.

R. A. BECHER

Heritage of the Dales



Limekilns are no longer worked in limestone areas of the English Lake District but the remains of these strange looking structures can still be seen, though it is rare these days to see a kiln in good repair. This fine specimen is to be found near Conistone where it is sited on a source of limestone which, records say, was being worked for agricultural lime at this point in 1690; this particular kiln is, however, only about 150 years old. Lime burning is one of many small industries which have occupied Lakeland dalesmen over the centuries. For the record, these activities are now effectively described by J. D. Marshall and M. Davies-Shiel in *The Lake District at Work* (David and Charles: Newton Abbot, March 1971, £2.75), a picture book which tells how clog making, the manufacture of woollens, bobbin turning, lead and copper mining and many other small industries formed, with agriculture, the life of the dales before the tourists moved in.

Chemistry for Schools

Nuffield Advanced Science. Chemistry. (Penguin: Harmondsworth, Middlesex, 1970. Published for the Nuffield Foundation.) *Students' Book II*: Topics 13 to 19. Pp. viii+308. £1.00. *Teachers' Guide II*: Topics 13 to 19. Pp. 354. £1.50.

THE recent *Black Paper Three* contained a remarkable attack by John Bradley of Hull University on the Nuffield O level Chemistry Sample Scheme. The Sample Scheme has many faults, as anyone who has given it an extensive trial can tell, and it is being revised; but, properly used, it gives insight into the key ideas of energy and structure. And my customers seem to prefer it to the older, more historical and descriptive, approach. But then they like to know why they are doing a particular experiment; and, far from being satisfied with mere description, they keep demanding explanations for phenomena until they get one which satisfies them. Bruner's dictum, that any subject can be taught in some intellectually honest form to any child in any stage of development, is very probably true—provided the child has a damn good teacher and the teacher hasn't got too many children. It retains some validity even in the non-ideal case of the average teacher with a large class. I have found to my surprise and pleasure that bright thirteen and fourteen year olds can, with the help of a few simple experiments of the type which Bradley calls "advanced and bizarre", readily grasp the essential structural features of plastics.

So far as I know, the Nuffield Advanced Chemistry course has not yet been the target of Bradley's faintly Messianic prose; no doubt it will strike him as even more bizarre. (His *Black Paper* polemic concludes with St Matthew, 7, verse 9. I retort with verse 19 of the same chapter, as justification for the elimination of methods and ideas which become outdated and unfruitful.) To me, the course seems a logical development of the aims stated in Brian Young's foreword to each of the books: a course which is satisfying and intellectually exciting in itself, with an emphasis on learning rather than being taught, on understanding rather than amassing information, on finding out rather than on being told. In my review of the first batch of the A level chemistry materials (*Nature*, **228**, 786; 1970) I criticized them on two grounds only: cost, and a conceptual intensity which might intimidate the less intellectually able. I'm still sore about the cost; but I'm glad to report that, over the past one and a half terms, we have found all forty-odd of our lower sixth chemists reasonably capable of absorbing the ideas.

This second batch of books maintains

the high quality of the first. I was delighted to see, in the *Students' Book II*, photographs of stamps commemorating Kekulé's proposal for the structure of benzene. An excellent brief historical introduction to dyestuffs begins with a quotation from Edmund Burke: "People will not look forward to posterity, who never look backward to their ancestors". (Perhaps this may mollify Bradley?) There is a good summary of acid-base chemistry in the human body. The section on detergents is illustrated by shots of some consequences of the Torrey Canyon disaster, and with one of the River Lee in its full 1961 vintage foam (nicely captioned "One result of the use of detergents with a branched chain alkyl group"). The writing throughout is concise, clear, and stimulating, although not completely free from trivial errors. It is a bit puzzling, though, that all the material on entropy is relegated to the *Teachers' Guide*: as the *Guide* states, "It is certainly desirable on general educational grounds that sixth form science students (perhaps all sixth form students?) should have some understanding of entropy". Sure: so why not put something about it in their book?

MARTYN BERRY

Hard Men and Hippies

Varieties of Juvenile Delinquency. By Carl Frankenstein. Pp. xi+252. (Gordon and Breach: London and New York, December 1970.) £6.00; \$14.50.

MOST psychologists have explained juvenile delinquency in a restricted way by applying psychological or sociological definitions: they either consider the problem to be the result of an interplay of social factors or hold psychic disorders solely responsible. In *Varieties of Juvenile Delinquency*, Carl Frankenstein analyses several types of delinquency including waywardness, psychopathy and drifting, and compares the causes and symptoms of each, explaining the presence of considerable variation in terms of both structural and socio-economic experiences.

The detailed comparisons of psychopathy and adolescent delinquency are used to show the fallacy of attempting to interpret the problems of adolescence as developmental psychopathy. Although there are superficially some similarities, notably the apparent senselessness of destructive acts, Professor Frankenstein discusses convincingly, and in considerable detail, the differences between the two behavioural categories.

A fascinating commentary is also given on the subject of post-war delinquency and the apparent development from the anti-social Teddy Boy type to the asocial drifter of the Hippy

cult. From the theme of this study it is hoped that the members of each of the groups will be considered in the light of their individual psychological backgrounds as well as the socio-cultural conditions of post-war western society.

The last chapter consists of a selection of most interesting case studies illustrating the various types of delinquent and offering analysis of their problems.

Although the book itself is of an essentially specialist nature, the ideas Frankenstein puts forward will be of value to a much wider group of people who come into contact with the juvenile delinquent and his difficulties.

DIANNE WOODHAM

Danger in Decibels

The Effects of Noise on Man. By Karl D. Kryter. (Environmental Sciences: an Interdisciplinary Monograph Series.) Pp. xvii+633. (Academic: New York and London, October 1970.) £9.10.

DR KRYTER has undertaken a sterling task in writing such a book. The subject matter, although lumped under a common title, ranges through many disciplines; medical, physical and social sciences, together with engineering. It gives the appearance of being a lifetime's accumulation of information. Who will find this book useful? Certainly many people will find sections of the book of great value. The number of graduate students in the UK who can use the book will be quite limited, but even so, it is doubtful if the standard of the text is of adequate depth. The engineer and environmental planner will find only an introduction to the topics of interest, although the bibliography will certainly be useful. The same impression also applies to the research worker.

The dust jacket states that the volume presents a critical and historical analysis of relevant literature. This is certainly true, although the author's bias is clearly detectable, particularly in Part II, "Subjective Responses to Noise". There are places in this section where the author, either in haste or through lack of understanding, doesn't quite explain another worker's results. For example, on page 396 Zepler and Harel did not calculate a unit proportional to Phons (Stevens). On a point of general philosophy, this section of the book lays great emphasis on the conceptual importance of noisiness and its assessor, the perceived noise level. The usefulness of this unit, particularly for assessing the acceptability of sounds produced by fixed wing aircraft, must be acknowledged, but its universality must be carefully questioned.

Because of the diversity of the book's contents, it is difficult to discuss detail without resorting to a large manuscript, therefore a general overview must suffice. In spite of its faults, which include an inadequate index, it is a remarkable book, by an authoritative worker in a field that sadly needs the discipline of publication.

J. B. LARGE

Exposed to Pesticides!

Aldrin, Dieldrin, Endrin and Telodrin: an Epidemiological and Toxicological Study of Long-Term Occupational Exposure. By K. W. Jager. Pp. 233. (Elsevier: Amsterdam, London and New York, 1970.) £3.50.

IN all discussions on pollution the role of pesticides never fails to receive mention. Dieldrin is one of the persistent insecticides which has fulfilled a very useful role in agriculture and public health. By the skill of the analytical chemists, however, we have learnt that measurable amounts may be present in our diet and, because of its relatively long half-life, it can be detected in our body fat. As it is impossible to determine whether or not such a small exposure as the British or American citizen receives (6–7 $\mu\text{g}/\text{man}/\text{day}$) is harmful, it is useful to study a population whose daily intake is 50–100 times greater and whose health and activity are under constant surveillance.

This book from the medical department of the Shell Refinery and Chemical Plant at Rotterdam contains a wealth of detail about the men who were exposed to dieldrin and the related compounds—aldrin, endrin and telodrin—during their manufacture and formulation. That these men were at times seriously exposed to these compounds is indicated by the fact that there were 34 people who developed convulsions and 54 who had other clinical evidence of intoxication of a less severe degree. There were no fatalities and no irreversible neurological changes.

Originally the exposed group were monitored by electroencephalogram determinations, but with the improvement in analytical methods and better knowledge of the metabolism and half-life of the compounds, obtained by planned studies on volunteers by UK scientists, the routine use of blood levels became the method of determining whether or not occupational exposure was excessive.

For the general reader the most significant section is the study of 106 men with an average of 7 years (4–13 years) continuous exposure chiefly to dieldrin, aldrin and endrin. No less than 20 clinical tests, ranging from body weight and blood pressure to the determination of half a dozen serum enzyme activities,

failed to show evidence of abnormality associated with exposure. By comparison with the refinery workers in the same plant, those exposed to insecticides showed no greater sickness absence.

While the medical reader may wonder why a manufacturer persisted for so long with a compound like telodrin when so much was already known about dieldrin, he can have nothing but admiration for the author who provides such a full and frank account of the health of the men under his care while making these insecticides. Although 7 years of exposure is not a life-time, nor 160 a great population, the fact that their daily intake was so much greater provides a good measure of reassurance for the general population ingesting their daily dieldrin. The company's management is to be congratulated on sponsoring the publication of this account and it is to be hoped that their example will be followed by manufacturers who have information on occupational exposure to other toxic chemicals.

The book includes a comprehensive summary of the toxic properties of insecticides derived both from experimental studies on animals and from accidental and planned exposure of man.

J. M. BARNES

Assessing Technology

Technological Change: Its Impact on Man and Society. By Emmanuel G. Mesthene. (Harvard Studies in Technology and Society.) Pp. ix+127. (Harvard University: Cambridge, Massachusetts; Oxford University: London, December 1970.) £2.40.

IN 1964, IBM endowed a programme of research at Harvard University on the impact of technological change on man and society. The title is important. Although lip-service is paid to reciprocity, the primacy of technological change is in fact the basic working assumption.

This short and readable little book does not report the results of the Harvard programme, but is rather an essay on the philosophy which underlies it. Starting with the now conventional critique of the optimists and pessimists with respect to technical change, Mesthene argues persuasively for a balanced assessment which recognizes both light and shadow. He also dismisses the third view, attributed to unidentified historians and economists, according to which there is really nothing new in technological change as it has been with us throughout history. He argues that the nature and scale of contemporary technological change distinguish it from earlier varieties and call for a special type of response.

In terms of contemporary American society, the response which he calls for is in fact a revolutionary one, but the

argument is presented in such an unprovocative, urbane style that nobody would notice; and it is scarcely surprising that the radical American left regard him as part of the establishment. He is, in fact, arguing that the private market mechanism is increasingly inadequate to cope with the consequences of modern technology, and for a greatly enhanced role for public decision-making in relation to the economy and technology. Because the free enterprise system cannot effectively assess or control the introduction of new technologies, and as their disruptive effect may be very great, this must be a public function. Indeed, according to Mesthene, it is already so to a large extent.

Unlike many other Americans, Mesthene does not view this prospect with dismay. The brave new world apparently holds few terrors for him, and he is optimistic about the possibilities of participatory democracy in relation to advanced technology. Although he makes perceptive comments on the wilful determinism of the pessimists, who have made up their minds to see only the negative side of technical change, he is himself vulnerable to the criticism that he skates too lightly over the pessimistic critique. Aspiring to a judicious balance between optimism and pessimism, he actually comes down decisively on the optimistic side. Occasionally this comes close to a facile optimism, as in his frequent failure to distinguish between the decision-making of the individual and that of the large corporation in relation to the economy and his omission of any real indication of how public decisions in relation to technology are to be made.

As, however, nobody else has solved these problems either, it would be unfair to be over-critical, and one must be grateful that the Harvard programme is approaching these problems in an inquiring and undogmatic spirit. Mesthene refers to a number of interesting projects in their programme which suggest that there is still some hope that technological change might one day become the dependent variable. But they obviously do not look at it like that.

C. FREEMAN

A Vital Statistic?

The Divine Proportion: a Study in Mathematical Beauty. By H. E. Huntley. Pp. xii+186. (Dover: New York; Constable: London, September 1970.) £1.25.

A SEGMENT AB is said to be divided internally at C in "the divine proportion" if $AB/AC = AC/CB$. This ratio, which the author calls ϕ , is then equal to $(\sqrt{5}+1)/2$ or, approximately, 1.618. The author has written a paean in praise of the divine proportion, drawing

the reader's attention to its ubiquity in human experience and in nature. It occurs in several contexts in mathematics (particularly, of course, geometry); it is to be found in music, art, architecture, morphology, phyllotaxis, to name but a few of its manifestations.

The subtitle of the book indicates the author's thesis—the divine proportion is primarily to be regarded as an example of mathematical beauty. The author devotes much attention to the psychological and aesthetic aspects of our appreciation of beauty and invokes various sources, many poetic, to support the view that the quest for beauty is the chief creative impulse in man.

With its wealth of examples, this book makes fascinating reading. The mathematical level is elementary, so that only the utterly innumerate need be daunted. The author's evident enthusiasm for his task, his dedication to beauty, and his catholic scholarship all command the reader's respect and predispose him in favour of

the author's viewpoint. Yet, in my opinion, the book is flawed by the author's very partisan approach. It may well be true that the search for beauty is the mainspring, for many, of their appreciation of existing mathematics; but no creative mathematician could fail to cite pure intellectual curiosity as a potent stimulus to his research. My difference with the author may be pinpointed in the latter's oft-repeated and central theme that creation and understanding are utterly akin. Indeed, he deduces the view that the quest for beauty is the chief creative impulse from this equation and his analysis of the appetite for understanding. He repeatedly quotes Jacob Bronowski in support of this theme (but Bronowski has created essentially no mathematics). Sir Peter Medawar has expressed, elsewhere, the contrary view; by no method of communication can one convey to another the thrill of discovery in science. The truth, for many, lies somewhere in between; something of

this thrill is conveyed, but something significant is lost.

The same partisanship mars some of the author's mathematical exposition; so keen is he to demonstrate the mysterious ubiquity of ϕ that he often obfuscates the issue. There is absolutely nothing surprising that, given the Fibonacci difference equation

$$u_{n+1} = u_n + u_{n-1}, \text{ then } \lim_{n \rightarrow \infty} \frac{u_{n+1}}{u_n} = \phi;$$

the theory here is standard and is not special to ϕ . There is nothing special, as suggested on p. 37, about starting with u_1, u_2 negative (though there is something special, not mentioned by the author, about starting with $u_1 = \phi^{-1}, u_2 = \phi^{-2}$). There are palpable examples of special pleading on pp. 65, 66; and the author's love of geometry has, perhaps, led him to over-state the case, emphasizing the purely aesthetic appeal of mathematics at the expense of its immense service in the interest of man's mastery of nature. PETER HILTON

Physical Sciences

Ultraviolet Astronomy

Ultraviolet Stellar Spectra and Related Ground-Based Observations. Edited by L. Houziaux and H. E. Butler. (International Astronomical Union Symposium No. 36, held in Lunteren, The Netherlands, June 1969.) Pp. xv+361. (Reidel: Dordrecht, 1970.) Hfl. 60.

AFTER a somewhat slow start—bedevilled by calibration problems as well as initial technical difficulties—ultraviolet stellar astronomy has come of age largely as a result of three principal developments: improvement of quantitative measurements (mostly by workers at the US Naval Research Laboratory and the NASA Goddard Space Flight Center) to the point where there are many firm data that can be taken as boundary conditions on theoretical models of stellar atmospheres; the identification (originally by the Princeton group using simple rocket-borne spectrographs with film) of stellar and interstellar spectral lines down to 1100 Å; and the acquisition by the Washburn Observatory, Wisconsin) of a wealth of quantitative data on both lines and continua with the aid of photoelectric scanners mounted on Orbiting Astronomical Observatory satellites. In addition, valuable data have been collected on interstellar reddening, broad-band photometry and galactic Lyman- α emission; and a number of interesting theoretical developments have been stimulated.

These and other advances are well

described in this book together with allied theoretical and ground-based papers so that the objects studied (by whatever technique) can be viewed "in the round". For example, absolute intensity calibrations seem now to be attainable to about ± 10 per cent accuracy anywhere between 1000 Å and 1μ . The interstellar reddening curve from 2μ to 1100 Å suggests some combination of ices, graphite and silicates, but there is no unique or wholly satisfactory model; and the continua of the stars themselves partly agree and partly disagree with predictions.

Stellar line spectra provide many angles for fruitful interaction between ultraviolet and ground-based work and (in addition to the ultraviolet papers) the book has interesting contributions by M. W. Feast, A. J. Deutsch and J. B. Hutchings on stellar chromospheres and mass loss, as well as discussions of fluorescence and other non-equilibrium effects. The final section on interstellar lines is chiefly concerned with Lyman- α in absorption; an apparent anomaly relative to 21 cm data is extensively discussed by the Princeton and Wisconsin astronomers and probably requires better data for its resolution. There are also a few papers on sky background emission (giving some evidence for Lyman- α from the Milky Way) and on ultraviolet emission from the Sun.

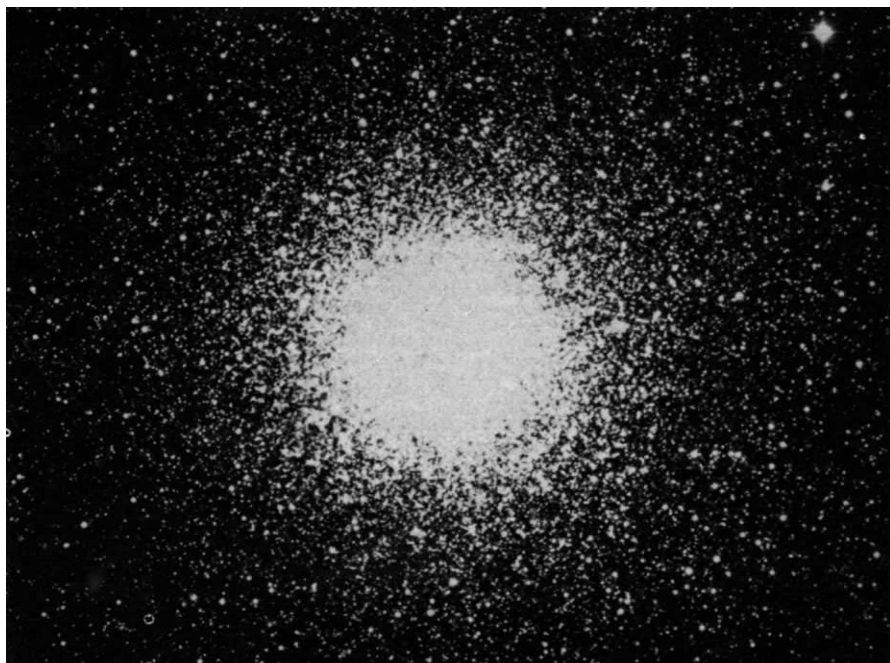
The idea of having this symposium was suggested by the late Armin J. Deutsch, whose premature death just after it had been held was a tragic loss

to astronomy, and it is appropriate that the book has been dedicated to his memory. In 1967, when he had the idea, the amount of ultraviolet results was quite meagre, but the timing of the symposium in fact proved to have been very happily chosen, coinciding as it did with the arrival of a great mass of new data. As far as the proceedings are concerned, several topics are of interest now that could not be included in the book because not enough was known about them at the time of the meeting; and conversely not everything in the book is of burning interest for permanent record purposes. Nevertheless, its production has been well worth while as a stimulating and fairly complete picture of ultraviolet stellar astronomy and of the various problems to which the technique is capable of contributing. BERNARD PAGEL

A Box of Stars

Catalogue of Star Clusters and Associations. Edited by G. Alter, J. Ruprecht and V. Vanysek. Second considerably enlarged edition edited by G. Alter, B. Balazs and J. Ruprecht. Pp. 3,86. (Akademiai Kiado: Budapest, 1970.) £15.00.

THE study of star clusters occupies a central place in the interaction between astronomical observation and theory. From photometry of its individual members, a cluster's colour magnitude relation can be determined. This can



M3, one of the brightest stellar globular clusters visible in the northern hemisphere. It may contain 500,000 stars.

be interpreted in terms of the distance and age of the cluster by comparison with the observed colour magnitude relations of other clusters, and with theoretical colour magnitude relations simulated from computations of the structures and evolution of a sample of stars of different masses. The distances and distribution of the clusters have been crucial to our tracing of the structure of the galaxy. While the very old globular clusters (see illustration) populate the galactic halo, the younger open clusters are concentrated to the galactic plane, where the very young clusters and associations delineate the spiral arms.

The extensive literature concerning star clusters makes the *Catalogue of Star Clusters and Associations* an indispensable research tool. This second edition of the *Catalogue* comprises more than 1,500 printed cards describing nearly 1,300 objects. Each object is catalogued on a separate card carrying its designations and position in galactic and equatorial coordinates, followed by the reference for every paper citing the cluster giving notes of the observations made and context in which the cluster was discussed. The most recent references are to papers published in 1968; but a comparison with the *Astronomischer Jahresbericht* showed that not all the relevant 1967 and 1968 papers have been included in the *Catalogue*. A few gaps in the literature of earlier years have also been detected. The nature of the *Catalogue*, however, readily allows these omissions to be filled, and further data to be added to each card. Following the publication of the first edition of the *Catalogue* in 1958, the editors issued (in the *Bulletin of the Astrono-*

mical Institutes of Czechoslovakia) nine annual supplements to the *Catalogue*, containing data to be transcribed to the cards. It is hoped that this practice will be continued, enabling users to maintain their copies up to date.

The second edition differs from the first not only in its size, but also in its organization. The open clusters and the associations are now arranged in order of increasing galactic longitude, befitting their significance to galactic structure. This makes it rather more difficult to find clusters by their usual designations; but correspondence between the new numbering and the NGC, Messier and older numbering is given in the 70 page explanatory booklet accompanying the *Catalogue*. Altogether, it seems very probable that this *Catalogue* will prove even more useful than its predecessor.

P. M. WILLIAMS

Optimization Techniques

Introduction to Methods of Optimization. By Leon Cooper and David Steinberg. Pp. vii+381. (Saunders: Philadelphia and London, 1970.) £5.30.

THIS text is intended as an introduction to optimization for the user, as distinct from the student, of mathematics. Optimization, in this context, means maximizing or minimizing functions of n real variables over regions in Euclidean n -space, for the authors exclude more advanced topics such as the calculus of variations and optimization in random situations—allegedly on grounds of space. What is left is linear programming, with extensions of the methods to quadratic and integer programming, all this occupying about half

the book, together with chapters on classical optimization (finding stationary values of functions) and on optimization by search techniques.

In view of the intended readership the selection of topics seems reasonable enough, and indeed it is valuable for the recently developed research techniques to appear in a text at this level. It is rather with the exposition that I must find fault. For, in proofs and worked examples, the authors attempt total explicitness in their calculations. Every step is stated, including many which any reader capable of understanding the ideas could do in his head. The result is, often, lengthy dullness, and sometimes confusion when the authors get so enmeshed in calculating particular instances that they forget to state a general principle, or when the mass of unnecessarily displayed formulae obscures the key ones. Particularly in the programming chapters, where the book faces strong competition from other texts at the same level, and where the ideas are easy but the notation complicated, is the authors' approach at its least satisfactory. In case these remarks are subjective, and prospective optimizers exist who would like this approach, one can only suggest sampling it, on shop or library shelves. But for me the book demonstrates that only up to a point (a point passed here) can explicitness aid mathematical understanding.

CHARLES GOLDIE

Mathematical Series

Infinite Series. By G. M. Fichtenholz. Course 3: Rudiments. Pp. vii+136; \$9; £3.75. Course 4: Ramifications. Pp. vii+130; \$9; £3.75. (The Pocket Mathematical Library.) (Gordon and Breach: New York and London, December 1970.)

THESE volumes have been translated from the relevant portions of Fichtenholz's large Russian treatise on calculus, to provide a practically self-contained account of infinite series.

The *Rudiments* volume contains enough material for those who require a minimal working knowledge of the subject. It deals first with series of positive terms, and the chief convergence tests, including the Kummer scale, Gauss's test, and Cauchy's integral test. For series whose terms need not be positive, Dirichlet's powerful convergence test is obtained, so that these two sections provide adequate utilitarian coverage. Infinite products are compactly handled and the final chapter discusses expansions in series and products of the elementary functions, chiefly by means of Taylor's theorem.

The second volume, *Ramifications*, demands slightly more sophisticated treatment. Riemann's theorem, that a

convergent series which is not absolutely convergent can be rearranged so as to converge to any prescribed sum, and the three main theorems on multiplication of two convergent series, exhibit a certain delicacy in technique as well as surprises in content, which can drive the novice into making sure that he has fully grasped basic concepts. So too can the short but good chapter on double series. For those who enjoy a little arithmetic, the processes given by Euler, Kummer and Markov for transforming a slowly convergent series into one which converges more rapidly provide pleasant excursions, particularly as the two latter offer high rewards for virtuosity. The final forty pages give an excellent introduction to the theory of summation processes, conveying a surprisingly large amount of information and thoroughly preparing the interested student for reading specialized treatises, such as Hardy's *Divergent Series*.

Double limit problems, such as arise in differentiating or integrating a series, are not mentioned: no doubt they will appear in the third volume, to be entitled *Functional Series*. Thus there is no serious need for the concept of uniform convergence. It is a pity, however, that the infinite product for the sine function is brought into the first volume, for at this stage it necessitates a long, tedious and far from illuminating proof; this is essentially a double limit problem, best coped with by Tan-ner's theorem, in the much simplified form of statement and proof given by E. H. Neville.

The reader should not neglect the exercises, well chosen, interesting, not too numerous, complete with answers and hints. The text is clear and readable; no doubt it cannot rival the stimulating brilliance of Bromwich or the methodical completeness of Knopp, but it provides a lucid if expensive introduction to a subject which is both useful and extremely elegant.

T. A. A. BROADBENT

Relativity on Trial

Relativity Re-examined. By Leon Brillouin. Pp. xi+111. (Academic: New York and London, October 1970.) £3.15.

THE task of the reviewer of a book like this is a difficult one. One feels oneself rather in a position of a theologian called upon to pronounce on the testament of an atheist which on closer inspection proves to be based on an astonishing intuitive understanding of the Old and New Testaments. The fact that Professor Brillouin sees fit to criticize a number of features of relativity both special and general should make those in the subject aware that all has not been so well presented as it

should have been. For this book shows that a most distinguished scientist can still come to a state of gross misunderstanding both of what the theory says and of how to put it right. It is more instructive to consider the latter feature of the book. Essentially Brillouin's view of modern physics can be summarized in the assumptions that since 1900 there are three basic results which we do not at all understand, but which we must incorporate into physics: Planck's relation between energy and frequency, Einstein's relation between energy and mass and the existence of stable energy levels. He sees the physicist's task as somehow to incorporate these results into a classical theory with which they are inconstant. As far as special relativity is concerned, his attempt to do this by means of an operational technique like Bridgman's leads to a reformulation of the theory which agrees in all observable respects with Einstein's original formulation. Brillouin's quarrel with Einstein is only about fairly minor matters of interpretation.

General relativity is quite another matter. Here the Brillouin theory would consist of the following non-linear theory of gravitation: Newtonian theory where the density of matter includes the energy density located in the field, calculated in the same way as is familiar electromagnetic theory. One would have liked to be able to ask Brillouin how he would square this theory with the requirement of Lorentz invariance, for in my opinion the result of doing this is to produce a theory which cannot differ more from the orthodox version of general relativity than Professor Fock's version (which receives a moderate degree of praise from Brillouin). On the whole an unsatisfactory book which does little to throw light on the theories that it criticizes, though one or two of the author's queries are very apposite.

C. W. KILMISTER

Photography under Water

In-water Photography: Theory and Practice. By Lawrence E. Mertens. (Wiley Series on Photographic Science and Technology and the Graphic Arts.) Pp. xiii+391. (Wiley-Interscience: New York and London, November 1970.) £9.50.

As the name implies, this book is intended to present the basic theory of "in-water" or "under-water" photography and to show how it can be practically applied. As such it is invaluable to photographers for whom it will provide a mine of information. It should not, however, be ignored by any technician, diver, engineer or designer who has any-

thing to do with under-water equipment or systems involving optical, photographic or even visual aids. In general, explanations have been simplified, but inevitably mathematical solutions do become involved; but for those who are not familiar with advanced mathematics, little should be lost from the general concept by omitting these sections. The extensive bibliography included with each chapter will enable specialists to develop as they wish any aspect which could not, of necessity, be covered in great detail in a single volume.

The first part of the book describes the water environment and the great variety of conditions which can be found at different times and places. The transmission of light in water and the way in which it is attenuated, selectively absorbed and scattered is thoroughly dealt with. From here it then explains how these factors affect the apparent contrast of objects and resolution of small detail, and gives possible methods of improving image quality in these inherently bad conditions. The merits of using filters, both selectively colour absorbing or polarizing, are discussed in connexion with both monochromatic and colour film.

Possible improvement in image quality by the use of supplementary lighting, the desired colour temperature and best geometrical arrangement in a system is explained, and this is followed by a review of some of the under-water lights commercially available on the American market.

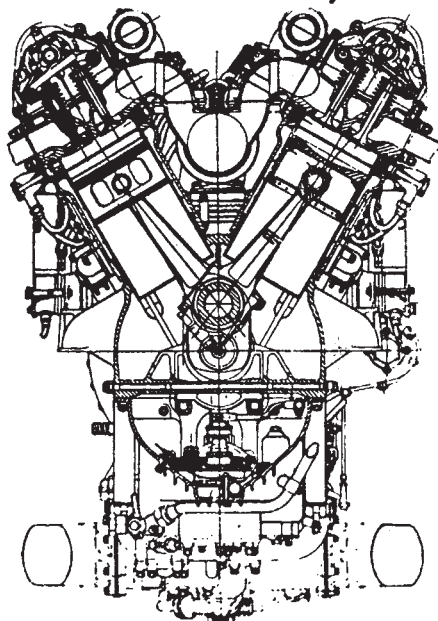
Chapter 6 deals with optical ports and lenses and quite adequately discusses the merits and disadvantages of plane-parallel ports, spherical ports and fully corrected optical systems. The next chapter on camera housings brings out handling points only too readily appreciated by the practical diver-photographer and thus emphasizes the need for good cooperation between designer and user. It covers the use and construction of both diver operated and remotely controlled units for housing still, cine and television cameras. We are next given a lesson in basic photographic principles, sufficient in detail to highlight capabilities and limitations. This is followed by a similar lesson on electronics dealing with television systems, image tubes, vidicons, image orthicons and image intensifiers.

The capability of the human eye and perception system to adapt itself to adverse conditions, and the problems this can create for the photographer, are discussed, together with a brief description of biological aspects of light and colour and of microscopic animal and plant life in the sea, the conditions in which they live, their reaction to light and so forth.

The final chapters deal with possible applications for photographic systems and then take us through the design stages of a hypothetical ocean floor population survey system, discussing and developing factors as they arise in the process. New techniques and possible future developments which could extend the present rather limited visible range by quite significant margins are discussed. Finally, there is a brief chapter on modern diving techniques.

P. A. SMITH

Heart of the Fairey Fox



Past glories echo present tragedies; the Rolls-Royce Kestrel VII pictured here was built in 1926 to power the Fairey Fox reconnaissance biplane; the government assured RR that it intended to use the engine in place of the American Curtiss D12—a ruse to encourage the firm to build the engine using its own money, while the government found more funds. From *The Power to Fly* by L. J. K. Setright (Allen and Unwin: London, April 1971.)

Cornerstones of Geology

Adventures in Earth History. Selected, edited and with introductions by Preston Cloud. Pp. xv+992. (San Francisco and Reading, January 1971.) £8.20 boards; £3.70 paper.

MANY teachers, and not only those of geology, take the view that there is more to be learned from reading original papers than from textbooks. The only drawback to this view as a teaching method is availability of a sufficient variety of papers to be studied by students. The idea for this book stems from the editor's preference for using published papers and the hitherto dearth of suitable "anthologies" which could be used as readers for group study.

Professor Cloud elected to narrow the field by restricting his choice of papers to those printed in English, which were not lengthy, could easily be

condensed and which posed no problem of copyright. While in the preface he says his choice "concentrates on North America", in practice the book is not parochial and as many of the papers serve only to illustrate concepts and ideas, the "place" is immaterial. Hence the book comprises eighty-three papers, including two by the editor, grouped into two parts, with five sections to each part, the first part dealing with fundamentals and origins of the science and the second with the geological record. Each section contains at least six papers, and there is a linking commentary in each case to provide continuity from one section to another.

The first section illustrates the basic principles of earth history and contains the earliest papers by Steno and Playfair written in 1669 and 1882 respectively. No earth history study would be complete without some reference to cosmology and the origin of our solar system, and the six papers in this section are as good an account as to be found anywhere. This section leads naturally into one concerned essentially with dating and correlation of the geological column and is one which has a bearing on the second part of the book. Quite what the precise significance of atmospheric and oceanic circulation has to do with earth history is not made clear and the section on climatology and oceanography is perhaps the least justifiable. The section dealing with the solid earth, evolution and drifting of continents, however, more than makes up for this. It is a pity, however, that the "cut-off" date for papers was 1968 as one on plate tectonics would have brought global tectonics up to date.

The second part of the book—the geologic record—contains the most (45) papers and deals with almost every kind of geological and evolutionary event from structure of the Alps to extinction. The evolution of life in all its forms, including man—who has a separate section—is a recurring theme and there is a strong stratigraphical and ecological bias to the palaeontology, and hence the reader can see just how interpretations of the "record" are made. What is a little surprising, however, is that nowhere is there any mention of volcanism or metamorphism and its attendant structures as pages of earth history. Extrusive and intrusive igneous rocks and metamorphic rocks are just as much a part of the geological column as sediments, as of course are the rocks upon which radioactive dating, mentioned in section III, is carried out, so that a paper illustrating their occurrence and significance would have been particularly relevant. Nevertheless the book does provide an almost unique opportunity for readers to broaden their familiarity with the geological literature.

JOHN BROOKS

Russian Physics

Theoretical Physics: an Advanced Text. Vol. 1: Theory of the Electromagnetic Field; Theory of Relativity. By Benjamin G. Levich. Translated from the Russian. Pp. xviii+395. (North-Holland: Amsterdam and London, 1970.) 74 Hfl.; £5.50; \$13.

SUCH Russian physics textbooks as have been translated into English have had the reputation of being solid, exhaustive and rather dull. The "physics for the butterfly mind" syndrome has never taken hold in Russia, and their forbidding wedges of print and equations are not usually enlightened by so much as a quotation from *Alice in Wonderland*.

This book is no exception. The first part of a four-volume text entitled *Theoretical Physics*, it contains little less experimental discussion than the equivalent English or American text on electricity, magnetism or relativity. The level is that of a good second year course in a British university. Maxwell's equations are introduced in the first chapter and applied to the usual special cases of electrostatics, steady currents and the like. The classical theory of radiation is treated in a very tidy way which is slightly unconventional in dealing first with the generation of dipole and quadrupole fields before describing the simple special case of plane wave propagation. The classical theory of the motion of charged particles and scattering is also treated in detail. The relativity section deals with special relativity to the same level, and the motion of particles in fields receives a thorough analysis.

As a reference text, this book is first class. It is clearly laid out, the standard of production is very high, and the translation is good. As a principal course text, it has the disadvantages of high price, a rather condensed presentation, and a tendency to refer to Russian rather than western texts for back-up material. The market for undergraduate electricity and magnetism textbooks is highly competitive, and this book is unlikely to replace any of the established course texts. Nevertheless, it is a very worthwhile addition to any physics library.

D. G. C. JONES

Direct Reactions

Direct Nuclear Reaction Theories. By Norman Austern. (Interscience Monographs and Texts in Physics and Astronomy, Vol. 25.) Pp. ix+390. (Wiley-Interscience: New York and London, November 1970.) £9.50.

LESS than twenty years have passed since the first recognition that nuclei undergo direct reactions; up to that time, many reactions were expected to

proceed with the formation of compound nuclei. Direct reactions, on the other hand, excite only a few degrees of freedom of the nucleus. The study of these processes has since provided us with a rich source of spectroscopic information concerning nuclei. Examples include the inelastic scattering of nucleons, deuterons and heavier projectiles and rearrangement processes, the most notable of which is the deuteron stripping reaction. The author has been identified from the beginning with the principal theoretical developments in the subject. Several review articles have already been written by him; but this book shows much more scope and provides us with a detailed account of the situation to date.

His view is essentially an optimistic one. Little notice is given to the full complications of the three-body problem. Dispersion theory is referred to; but it is recognized as being of limited benefit in the understanding of these

processes. Instead, great stress is placed on the need to establish simple models and to deal accurately with their consequences. Much of the book is given over to the usefulness of the distorted wave method, which has been readily accepted by experimentalists and has enjoyed a great measure of success. Generally, there is now easy access to several computing programs based on this method. The successes have prompted the investigation of a variety of complications, ignored in early versions of the theory. The middle section covers most of these developments. We find lists of many references and it is most interesting that many of the papers have only appeared during the late 1960s.

The latter part of the book moves into deeper waters covering such aspects as the need for antisymmetrization and we see in certain instances that a proper handling of the transfer of a nucleon to a nucleus brings in features characteristic of nuclear-structure calculations.

We learn that exchange effects and the possibility of knock-on are not to be ignored, particularly in inelastic scattering processes. A proper theory requires a uniform treatment of the possibilities of direct reactions and compound-nucleus formation, and this is also discussed. The complications associated with nuclear spin tend to be avoided. Yet polarization measurements are now more common and the distorted-wave method shows major shortcomings here, at least when dealing with the stripping process. Whether this owes itself to ignorance about spin-dependent distortions or to inadequacies of the theory is not yet clear. One feels that more stress should have been placed on this feature at an earlier part of the book.

Austern's book clearly will become a standard reference for students and research workers in the field and it is a pity that its prohibitive price will deny it the wide circulation it deserves.

L. J. B. GOLDFARB

Biological Sciences

Molecular Toxicology

A Symposium on Mechanisms of Toxicity. Edited by W. N. Aldridge. (Biological Council: The Coordinating Committee for Symposia on Drug Action.) Pp. xiii+257. (Macmillan: London and Basingstoke, February 1971.) £5.50.

WHY is a given compound toxic? This is a question to which some kind of answer can be given from a number of different aspects or levels. One can say that a compound is toxic because it produces certain deleterious symptoms, destroys certain cells, produces a toxic metabolite or inhibits an important enzyme. The answer depends on whether one is thinking in terms of the whole animal, a tissue, a cell or a molecule. Academically the most satisfying and useful answer is that given at the molecular level, for here one is concerned with the mechanism of how the poison works. This volume is concerned with the molecular level and is a collection of papers given at a symposium organized by the Coordinating Committee for Symposia on Drug Action of the Biological Council. All the papers are very interesting, particularly to the biochemist interested in toxicity; although they are of high standard, they are likely to be of more interest to the specialist than to the general reader.

The book is divided into four sections containing four papers each with discussions. The four sections deal with the effects of poisons on enzymes, their

effects on proteins, the causes of cell injury and the problems of lethal synthesis. The first section contains papers on monoamine oxidase inhibition, hypothermia in rats by anticholinesterases, and glutamine synthetase. The papers on monoamine oxidase illustrate not only the complexity of enzyme inhibition but also its possible application. Isoenzymes of monoamine oxidase occur which have different substrate specificities, different responses to inhibitors and, as far as the brain is concerned, different inhibitor characteristics in different areas of the brain. The interesting suggestion is made that the synthesis of specific inhibitors tailored to an individual isoenzyme at a particular anatomical site should be possible. The paper on the inhibition of glutamine synthetase by methionine sulfoximine is an excellent example of the application of enzymology to toxicology. Only one of the four diastereoisomers of methionine sulfoximine (L-methionine-S-sulfoximine; S=sinister in the R, S stereochemical convention) inhibits the enzyme and only this isomer produces convulsions in mice. These findings strongly support the view that the toxic effect of methionine sulfoximine is related to glutamine synthetase inhibition. That methionine sulfoximine was identified many years ago as the toxic agent in agenzized wheat flour (agene=nitrogen trichloride), however, is not mentioned.

The second group of papers deals with the mechanism of toxicity resulting from reactions of toxic compounds with

proteins. Lathyrogens, the classical example of which is aminoacetonitrile, a component of the neurotoxic agent in the seeds of the sweet pea, seem to produce their effects by inhibiting cross-linking in collagen and elastin. Aspirin has the ability to acetylate proteins such as albumen in which the lysine residue is acetylated. The possibility that aspirin intolerance may be the endstage of a slowly progressive accumulative acetylating effect, and other consequences of protein acetylation such as initiating autoimmune phenomena, is discussed. There are two interesting papers in this section on the combination of toxic metals, particularly beryllium, with proteins. Beryllium seems to go for the cell nucleus and the nuclear proteins.

The third group of papers deals with cell injury, one on beryllium again and the possibility of its interfering with DNA. Attention is also drawn to the role of the nucleus in cellular damage, especially the effect of certain poisons on RNA polymerases in the nucleus. There is also an interesting paper on cell suicide and cell death in which attempts are made to answer the question "what do you have to do metabolically to a cell to kill it?". It is pointed out that the idea that the release of enzymes from lysosomes may cause cell death has little support; the enzymes from lysosomes work only after cell death. The possible advantages of *Amoeba proteus* as a cell model in toxicology is discussed in the last paper of this group. This unicellular organism can be selected for treatment with a

poison at periods of known chemical activity in its life.

The last group of papers is entitled "Lethal synthesis", a term coined by Peters to cover the biosynthesis of the toxic fluorocitrate from fluoroacetate. Accounts are given of lethal syntheses involving 6-aminonicotinamide and its synthesis into abnormal pyridine nucleotides, and hypoglycin, the hypoglycaemic agent in unripe ackee, a fruit eaten in Jamaica, and its synthesis into abnormal CoA derivatives. The roles of the liver and of the gut flora in the production of toxic molecules from relatively inert precursors are also discussed in two papers in this section.

All the papers in this symposium are of considerable interest not only because they illustrate mechanisms of toxicity at the molecular level but also because the hypotheses proposed and the questions raised give useful leads for future research which could provide rational explanations of the responses of man and animals to the toxic effects of drugs and other chemicals.

R. T. WILLIAMS

Genes in Tandem

Evolution by Gene Duplication. By Susumu Ohno. Pp. xv+160+8 plates. (Allen and Unwin: London; Springer-Verlag: Berlin and New York, March 1971.) £4.20.

ONE of the liveliest controversies in modern biology concerns how to explain discrepancies between estimates of the rate of evolution at the molecular level and the pace at which the evolution of species must have proceeded. The thesis of Dr Ohno's book is that gene duplication constitutes a principal mechanism for achieving rapid evolution.

The essence of his argument is that developing new functions by mutating existing genes so as to modify the proteins which they code is not likely to be an important force in evolution; once a gene product has acquired some definite function, mutations will probably interfere with that function and will therefore be harmful to the organism. Evolution by this means must therefore be exceedingly slow, if not impossible. But if a gene is duplicated, Dr Ohno argues, one copy can continue to specify the protein needed, and the other gene can be rapidly mutated and its protein product adapted to some new function, without any deleterious effect on the organism.

To sustain this theory, Dr Ohno draws on a remarkable range of fields, from molecular biology to the analysis of fossil remains. His synthesis of so many topics into one coherent theme is indeed impressive, but inevitably reliance on so many different types of observation leads in some instances to

too uncritical an acceptance of data, and to certain inaccuracies.

The book opens with a condensed review of nucleic acid and protein synthesis both in bacteria and in higher organisms. Discussion is too sketchy at several points. For example, why should tRNA have been present at the evolution of the genetic code? This is assumed too readily and ignores arguments that there may have been some physical interaction between primitive polynucleotides and polypeptides. And it should be said also that tRNA is itself a highly evolved molecule. The degeneracy of the genetic code is considered, but the wobble hypothesis to account for the mode of codon-anticodon recognition is not discussed.

An idea too easily accepted is that what are defined as "same sense" mutations (changing one codon for another which specifies the same amino-acid) could affect the rate of translation; this idea has been mooted many times, but it is important to say that there is no evidence to substantiate the suggestion that this may provide a mechanism for the control of translation. And the role of histones as repressors of gene action in higher organisms is greatly oversimplified. Nor is it immediately clear why the ability of histones to bind to DNA should require the maintenance of a fixed amino-acid sequence. It is admirable that Dr Ohno should draw on so many different concepts, but it is important to distinguish between those which are well established and those which are more speculative.

The second section of the book concerns the effects of mutation and discussion is generally good. One peculiarity is that no mention is made of "lethal" mutations, but much play is made with the "forbidden" variety; I cannot help but feel that this term is somewhat misleading, because such mutations can of course occur, although they may have deleterious effects. Any work on this topic must to some extent be concerned with the issue of neutral mutations. It is perhaps unfortunate that Dr Ohno only touches on the controversy as to whether mutations can be totally without selective value; a more detailed discussion of this point would have been useful.

The next two sections of the book are concerned with the need for gene duplication as an evolutionary mechanism and how it may have occurred. The case is at its strongest when discussing alterations to structural genes, and a fair range of examples is cited; trypsin and chymotrypsin; myoglobin and haemoglobin; the L and H genes of immunoglobulin; and microtubule protein and actin of skeletal muscle. But any talk of regulatory genes in higher

organisms, analogous to those of bacteria, is at present so speculative that the limitations of this idea should be outlined and it is important not to overlook them.

The final section of the book is devoted to explaining the evolution of vertebrate genomes in terms of Dr Ohno's theory. It is fair to say that not everyone will agree with the conclusions which he reaches. Nevertheless, he has presented an interesting idea, on the whole skilfully, which will doubtless be valuable not least for the controversy which it will stimulate. This is certainly a book to provoke thought, and it can be recommended to anyone interested in evolution at the molecular level. BENJAMIN LEWIN

Biochemistry of Fat

Brown Adipose Tissue. Edited by Olov Lindberg. Pp. xiv+337. (American Elsevier: New York; Elsevier: Barking, 1970.) £11.50.

THE editor's preface to the volume emphasizes the unique properties of the brown adipose tissue as a relatively simple biological material in which the physiological and biochemical characteristics can be related to its specialized function. The thermogenic property of the brown adipose tissue associated with its unusual anatomical features, and the fact that it provides a suitable biological material in a relatively simple form (isolated cells) to study some hormonal effects, has indeed stimulated an impressive amount of work during the last decade, and Professor Lindberg has performed a valuable task for biologists and biochemists interested in this field in producing the monograph.

The function of "non-shivering" thermogenesis has been shown to be associated with conditions in which body temperature has to be maintained and other thermoregulatory mechanisms are inadequate; such conditions exist in newborn animals where the thermoregulatory mechanisms are not fully developed, in hibernation where they are inadequate, and in animals kept at low temperatures where the heat requirement is abnormally high. The studies described in the present volume include investigations of brown fat tissue from the three above mentioned groups.

The volume consists of 13 articles (written by 22 specialists), each giving a comprehensive survey of the present state of knowledge covering the morphological, embryological, biochemical, electrophysiological and endocrinological aspects of the subject. Each chapter has an extensive bibliography of considerable value to any investigator wishing to become acquainted with the field. The task of

reading this monograph is greatly facilitated by the excellent way in which the volume is produced and illustrated, including some electron micrographs vividly illustrating the cellular components.

The only criticisms I can make of this publication are those which apply in general to a collection of review articles of this kind written by different authors and dealing with various aspects of research on a particular topic. In the first place it is always extremely difficult under these circumstances to avoid repetition of material. For example, in the present monograph the work which led to the recognition of the brown adipose tissue as being a major site of heat production is referred to a number of times, and likewise a description of the microscopic anatomy of the tissue is given by several authors. Secondly, there is, in some respects, a lack of integration in the separate articles dealing with closely related topics as, for example, the last four chapters dealing with the fascinating problems of the mitochondrial activity of this tissue and the special mechanisms by which the energy released in substrate oxidation is converted into heat and not stored as high energy phosphate bonds in ATP as in other tissues. In my opinion this general topic could, perhaps, have been better covered in one single chapter from which a clearer picture of the present state of knowledge of this complex field (which is obviously still very incomplete and the conclusions are, in some respects, controversial) could be obtained.

For a biochemist with physiological interests it is reassuring to be aware that there are still many investigators actively engaged on research into biochemical mechanisms in relation to physiological function; the emphasis today in the biochemical literature is on studies on isolated systems unrelated not only to intact organisms but even into intact cells, and for this reason I found reading this monograph a stimulating and welcome change.

ANNE BELOFF-CHAIN

Invertebrates *in vitro*

Invertebrate Organ Cultures: Colloquium on Experimental Embryology, Clermont-Ferrand, April 1968. (Documents on Biology, No. 2.) Organized by H. Lutz. Pp. xiv+252. (Gordon and Breach: New York and London, December 1970.) \$22, £9.25 boards; \$7.50, £3.15 paper.

THIS book is the second in a series designed to provide review articles for students and their teachers. It contains papers originally read at a conference held in 1968, most, but not all, of which

deal with morphogenesis and cell differentiation in organ culture. It would be wrong to expect a very coherent picture to emerge. Insects, molluscs, echinoderms, crustaceans and coelenterates have invited different kinds of question and present different obstacles to enquiry. It is a pity, but no fault of the authors, that the common interest striking the reader most forcibly is a technical one. Of some of the articles it would certainly be fair to say that the culture medium is the message. This is above all true of the major technical survey of methods by N. Le Douarin.

Nevertheless, it is clear that invertebrate organ culture, which has in the past lagged behind work with birds and mammals, is at the beginning of a very promising period. For coelenterates and planarians (here treated by L. Gomot and C. Ziller-Sengel) it can contribute to morphogenetic problems that now have classical status—metaplasia in the hydroid and determination of regeneration blastemata in the flatworm. In other invertebrates culture work is particularly helpful in clarifying the endocrine control of morphogenesis. The articles by L. Gomot and by P. Lubet and W. Streiff establish this for molluscs. Insects are treated by T. Lender and by P. Nardon and G. Plantevin, crustaceans by J. Berreur-Bonnenfant and echinoderms by R. Delavault and J. Bruslé.

As its editors intend, this volume will be of real value to readers not already involved in the field. D. R. NEWTH

Chemical Triggers

Chemicals Controlling Insect Behavior. By Morton Beroza. Pp. xii+170. (Academic: New York and London, October 1970.) £4.65.

THE six articles by sixteen authors published in this book were first presented in 1969 at an American Chemical Society Symposium of the same title. Two other books, *Control of Insect Behavior by Natural Products* edited by David L. Wood, Robert M. Silverstein and Minoura Nakajima, and *Chemical Ecology* edited by E. Sondheimer and John B. Simeone, also published by Academic Press in 1970, cover the same ground somewhat more comprehensively. All three books reflect an awareness on the part of many biologists and chemists in the United States, Japan and Canada of the need to find safer and more efficient alternatives to the broad spectrum pesticides still in use.

Although the importance and potential economic application of naturally occurring chemicals, including pheromones and allomones, for controlling the spatial and temporal structure of

populations have long been realized by some ecologists, the progress of this research field has been slow. The pace has accelerated now that the importance of an interdisciplinary approach, involving chemists and biologists, has been realized, and made more attractive by the recent technological advances which permit the rapid characterization and purification of natural products in micro quantities. This book does not attempt to give a comprehensive coverage of the subject matter, and as there has been no serious attempt at an integrated approach it loses some of its value. Not unexpectedly, the emphasis is on chemistry rather than on behaviour. The first chapter deals with sex pheromones in the Lepidoptera, the second with the so-called sex pheromones of the beetle *Ips confusus*, and the third with the sex attractants of the boll weevil *Anthonomus grandis*. The factor which attracts *Ips confusus* is described as a pheromone despite the fact that it is diet dependent and that its site of production has not yet been identified. It is evident that difficulties were encountered in the behavioural assays because of synergistic and antagonistic effects, and that there were problems of standardizing stimulus application, the receptor animals and the response. The chemistry is at a more sophisticated level than the behavioural work, and a great deal more work on behaviour needs to be done to explain, for example, why one pheromone is a powerful male stimulant in the laboratory but not a long range attractant in the field.

Professor Blum's review of the pheromones of social insects underlines the fact that a great deal more is known about these pheromones and their mode of action both as primers and releasers. It might be suggested that investigations into the primer effects of pheromones in non-social insects might repay study. The fifth chapter gives a good comprehensive review of the biochemistry of arthropod defensive secretions, but as the behavioural content is minimal the link with the title of the symposium is somewhat tenuous. The last chapter, by Dr Beroza, succinctly summarizes recent development and current usage of insect attractants and repellents in the United States Department of Agriculture. The support that this work on attractants gives to the stereochemical theory of olfaction is discussed briefly. The thesis that "the greater the number of chemicals tested the better the chance of finding a potent attractant" could be challenged. An indiscriminate search for attractants might well be wasteful of resources, and behavioural studies carried out on the responses of animals to chemicals in their natural environments might be more profitable. The layout, photo-

graphs and graphical representations are good and there are excellent reference sections at the end of each chapter. In spite of its shortcomings, this is a useful book for all those interested in the chemistry of natural products, pest control and in the new discipline of chemical ecology. J. D. THOMAS

West Indian Butterflies

Butterflies of Trinidad and Tobago. By Malcolm Barcant. Pp. 314+28 plates. (Collins: London, February 1971.) £2.75.

BUTTERFLY books can be divided broadly into two classes according to their degree of specialization. There are, on the one hand, those intended for the specialist, the genuine entomologist, and, on the other, books written primarily for the collector who wishes to be able to identify what he has caught fairly rapidly, to know where to look for the ones he has not yet caught, the best time of year to see them on the wing and something of their life histories. This latter class may be termed the more "popular" books and Malcolm Barcant's book falls into this class, although this is not to say that its scientific standard is not high.

While there are popular books on the butterflies of Africa, Europe, Australia and North America, no one so far has tackled the enormous task of setting out a pictorial, reasonably cheap edition which deals with South America. Yet the rain forests of the Amazon contain many of the most exotic, certainly the largest number of species of any region in the world. Quite a number of species on the South American mainland, however, "spill over" into Trinidad and this fact will undoubtedly make a book on the butterflies of Trinidad of interest to collectors of the world's butterflies.

It should be remembered, however, that this book is written primarily for the collector who is actually in Trinidad, for its general layout will strike the foreigner as rather odd because they are grouped first by habits and habitats, for example, "the water drinkers", "the shade dwellers", and then by rarity. Thus a specialist in *Heliconius* must search in different parts of the book for closely allied species. Although there is a check list of all species, there is no alphabetical index and this is at times inconvenient. The classification by rarity applies only to Trinidad, for some of the species listed as of "exceptional rarity" are common on the mainland as evidenced by the fact that they can be bought in England for a fraction of a pound. The 16 colour plates are excellent, and one wishes that the other 12 plates which are photographs on glossy paper had also been in colour. At the same time one cannot help feeling that

some of the illustrations have been "wasted", for example, there are two species of *Prepona* illustrated topside only. Identification of these species lies primarily in the undersides, which are described as "two shades of brown" and "totally fawn". These descriptions could fit any of half a dozen other species of *Prepona*. Two other species of *Prepona* not illustrated underside are described as "silver with irregular black lines" and "silver with indistinct black markings". The individual illustrations on the plates are not numbered but separate outline sketches on the facing pages are numbered and this can cause the reader extra labour and possibly slight annoyance.

In spite of these few shortcomings the book will undoubtedly be popular among collectors and lovers of these gorgeous insects whether or not they live in Trinidad.

E. R. LAITHWAITE

Traces in Time

Principles of Paleontology. By David M. Raup and Steven M. Stanley. (A Series of Books in Geology.) Pp. x+388. (Freeman: San Francisco and Reading, March 1971.) £4.80.

THE authors and publishers are to be congratulated on producing this lucid introductory palaeontology textbook which should have universal appeal. Its emphasis, which is clearly outlined in the preface and on the dust jacket, is directed at explaining the principles on which palaeontological concepts are formulated. It guides the reader progressively from mastering the techniques of analysing simple case histories to grappling with the more abstruse evolutionary tenets.

Just under half the volume is concerned with the description and classification of fossils. This is introduced by an extensive quantitative evaluation of the chances of preservation throughout time, followed by an extremely clear discussion of ontogeny and its significance. From thence the unitary value of the fossil population is discussed in terms of Recent analogies which lead naturally to an introduction of the species concepts. Having thus stimulated the reader, the remaining 32 pages of this section give instruction on how to categorize and identify materials to professional standard.

The second half of the book is devoted to the uses of palaeontology. This is introduced by a theoretical discussion of adaptation and functional morphology which is carefully based on a blending of biological observations with mathematical principles. In this chapter a major palaeontological advance is made by showing how the coiled "shell" form may be related to the analysis of the logarithmic spiral as

exemplified by an analogue computer model. From thence we progress to the influence of mechanics in terms of dinosaur jaw movements, problems of trilobite vision, and the life habits of cephalopods, brachiopods and bivalves. The succeeding chapter on palaeoecology, which is extremely competent, discusses the Recent ecosystem as its reference point and shows how the resulting principles can be applied to the fossil record. Equal weighting is given to the succeeding section on evolution which, though more theoretical in its treatment, is copiously illustrated with appropriate line diagrams. But, by comparison, its practical counterpart on biostratigraphy, which follows the evolutionary chapter, is surprisingly succinct. As explained in the preface, the final chapter on palaeontological data in geophysics and geochemistry is tantalisingly brief: but this is perhaps inevitable in that at the time of going to press this was still a novel research field fraught with problems inappropriate to the beginner.

Each chapter is introduced by a key statement of intent, subdivided by appropriate headings, and closed with a paragraph on the state of knowledge and assessment of the potential of the theme. This is followed by a suggested reading list of four to six texts which are particularly relevant to that chapter. Thus the book is laid out to conform to American academic educational standards at their best. Its literary style is similarly particularly suited to such an audience, which may render it a little difficult to the non-English-speaking reader. But the contents of the book leave no doubt that it is without rival in international circles; it is conceptually challenging, visually pleasing and easy to read, with a reliable cross index system. The bibliography contains reference to 241 texts of which more than 90 per cent are in English language publications (13 texts in German and two in French), with a heavy bias to recent information (36 per cent published during 1965–1970, 24 per cent between 1960–1965, 26 per cent in the 1950s, and 14 per cent pre-1950).

It is difficult to find fault with this book, which has fulfilled the objects of its preface: allometry (pp. 60–61) is passed over rather hastily, "shell" structure is largely omitted, reference to "the Cretaceous 'Chalk' of England" (p. 279) should probably read—chalk—not—"Chalk", and the definition of stromatolites as "typically layered cabbage-like structures" (p. 215) could cause confusion for those expecting to find a stalk. But these are minor problems. This textbook should be within the means of the serious student who will be frustrated at finding the library copies in continuous use.

JULIA A. E. B. HUBBARD

Continued from page 296

effect of the ocean is supposed to impose a uniform electric field at the edge of the shallow seas; two cases (I and II) therefore arise: in case I the inducing electric field causing leakage currents is south-north along the western boundary, and in case II it is east-west along the northern and southern boundaries. On other ocean-sea interfaces the continuity into the ocean of the tangential component of electric field is preserved; for such a large conductivity contrast this implies that the current streamlines there enter the ocean from the sea at right angles to the interface. The results obtained from cases I and II may be added vectorially by assuming a direction and amplitude for the inducing oceanic electric field. Figs. 1 and 2 illustrate the streamlines of electric currents in the seas, and the isolines of their (vertical) magnetic fields, in gammas, inland. The basic unit of length $L=111\text{ km}$ (1° of latitude) is used. In the numerical solution we use a graded square lattice varying in size from L to $L/16$. The Biot-Savart law is integrated exactly over each separate square of the lattice so that an accurate approximation is obtained even near the coast where the induced vertical magnetic field has a logarithmic discontinuity. The surface electric current j is scaled so that j/L is unity at the appropriate edge of the system, and the differences between numbers attached to streamlines (and coastlines) are proportional to the total currents flowing between them. The magnetic field is scaled differently, in order to avoid the possibility of misinterpretation. We use an estimate of $v\sim 10\text{ cm s}^{-1}$ for the ocean tidal streaming velocity, yielding an electric field $vB\sim 4.3\text{ e.m.u.}$ Taking the integrated surface conductivity of the sea to be $3\times 10^{-7}\text{ e.m.u.}$ gives a surface current of $0.13\times 10^{-5}\text{ e.m.u.}$ in the sea on the boundary with the ocean. As these are rough figures⁶ v may be larger. On the other hand, internal short circuiting in the ocean will certainly reduce the electric field from the estimate vB . We have therefore rounded off the current-magnetic field scaling factor to $0.15\times 10^{-5}\text{ e.m.u.}$ All magnetic fields in Figs. 1 and 2 arise from the electric currents shown there, but they are multiplied by $0.15\times 10^{-5}\gamma$. We also note that the induced magnetic field becomes infinite on the coast, but this is difficult to show on the diagram.

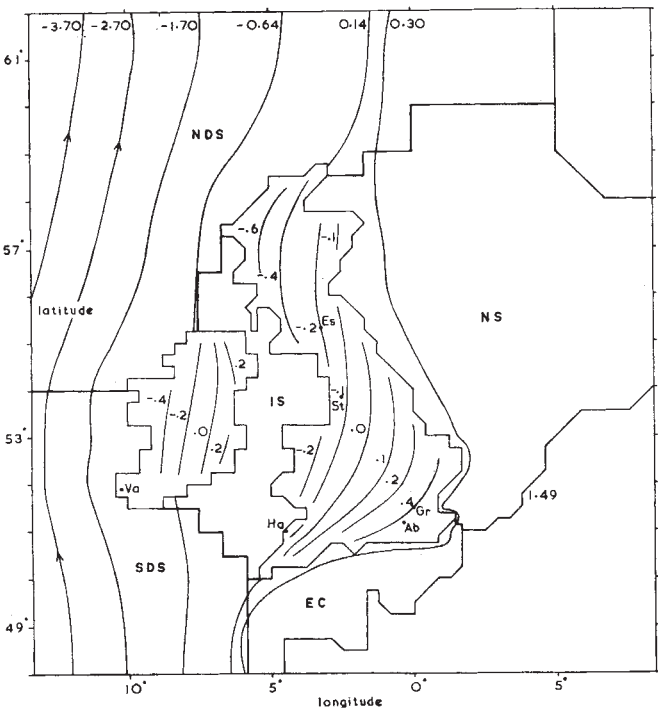


Fig. 1 Case I (see text). The electric current streamlines in the seas (for units see text) and the isolines of vertical magnetic field intensity (in gammas) inland.

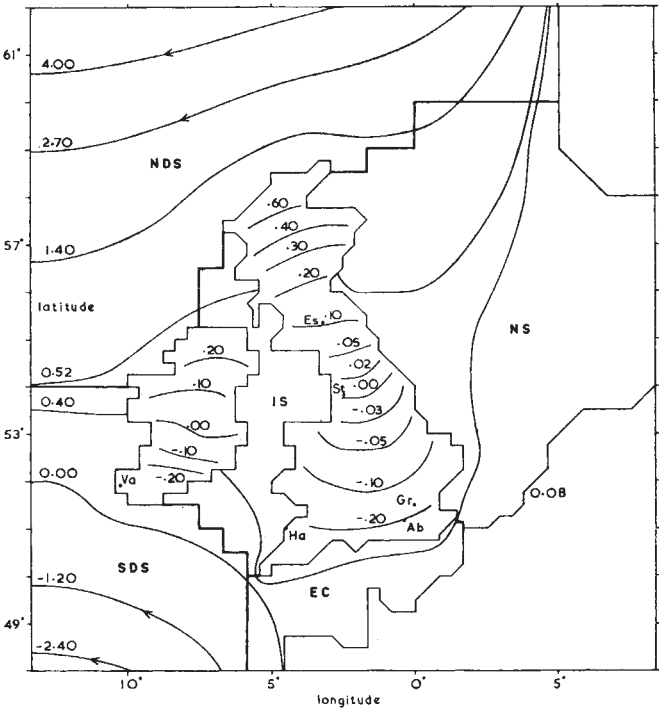


Fig. 2 Case II; otherwise as for Fig 1.

Tables 1 and 2 show in more detail the magnetic effects arising at stations within the British Isles from electric currents flowing in the various shallow seas. For this purpose we have divided the seas as shown on the maps into the Irish Sea (IS), English Channel (EC), North Sea (NS), Southern Deep Sea (SDS) and Northern Deep Sea (NDS). The results are scaled as before. We note the comparatively strong effects at coastal stations such as Hartland and Valentia, the relatively strong EC-NS effects at Abinger and Greenwich, and the IS-NS cancelling effects at Eskdalemuir in the case of south-north currents (case I). The difference between the amplitudes of the vertical lunar semidiurnal magnetic field variations observed at Greenwich and Abinger⁷ is 0.41γ . This is four times larger than the theoretical difference which we have calculated here from leakage of tidally induced electric currents arising in the Atlantic Ocean. It seems, then, that leakage currents cannot

Table 1 Case I: Vertical Magnetic Perturbation calculated for Various Stations within the British Isles						
	SDS	NDS	IS	EC	NS	Total
Abinger	-0.07	-0.05	-0.04	0.47	0.13	0.44
Eskdalemuir	-0.05	-0.17	-0.13	0.03	0.12	-0.20
Greenwich	-0.06	-0.05	-0.04	0.32	0.20	0.37
Hartland Pt	-0.17	-0.06	-0.28	0.21	0.04	-0.26
Stonyhurst	-0.06	-0.11	-0.12	0.05	0.12	-0.13
Valentia	-1.46	-0.07	0.06	0.03	0.02	-1.41
Ab minus Gr	-0.01	0.00	0.00	0.15	-0.07	0.07

See text and diagrams for division into shallow seas.

Table 2 Case II						
	SDS	NDS	IS	EC	NS	Total
Abinger	-0.04	0.06	-0.01	-0.17	-0.03	-0.19
Eskdalemuir	-0.04	0.18	-0.01	-0.01	-0.01	0.11
Greenwich	-0.03	0.06	-0.01	-0.12	-0.05	-0.16
Hartland Pt	-0.13	0.07	-0.15	-0.11	-0.01	-0.33
Stonyhurst	-0.04	0.11	-0.02	-0.02	-0.02	0.01
Valentia	-0.63	0.09	0.01	0.00	0.00	-0.54
Ab minus Gr	-0.01	0.00	0.00	-0.05	0.02	-0.03

Measurements were as for Table 1.

be completely ruled out in connexion with lunar semidiurnal variations, but they may be small. The faster streaming velocities observed in the shallow seas and the regions bordering on the continental shelf make it easier to produce somewhat larger effects more locally, and these are under investigation.

We note that if due allowance is made for inductance effects, parts of our model may approximate the situation in which electric currents are induced in the shallow seas by more rapid geomagnetic variations.

We thank Miss L. C. Thackeray and Miss C. Wright for assistance.

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¹ Hill, M. N., and Mason, C. S., *Nature*, **195**, 356 (1962).

² Larsen, J. C., *Geophys. J. Roy. Astron. Soc.*, **16**, 47 (1968).

³ Malin, S. R. C., *Planet. Space Science*, **17**, 487 (1969).

⁴ Kendall, P. C., and Chapman, S., *Q. J. Mech. Appl. Math.*, **23**, 535 (1970).

⁵ Chapman, S., and Kendall, P. C., *Planet. Space Sci.*, **18**, 1597 (1970).

⁶ Pekeris, C. L., and Accad, Y., *Phil. Trans. Roy. Soc.*, **265**, 413 (1969).

⁷ Leaton, B. R., Malin, S. R. C., and Finch, H. F., *Royal Observatory Bull.*, No. 63 (1962).

Terminal Cretaceous Events

SEVERAL explanations have been offered (see refs. 1–3) for the abrupt faunal extinctions at the end of the Cretaceous and, with the advent of the JOIDES Deep Sea Drilling Project, it was hoped that the nature of the extinctions—at least for calcareous microfossils—would be found by coring a transitional sequence across the boundary in deep sea facies. Unfortunately, the results from the few JOIDES holes so far penetrating the Cretaceous–Tertiary boundary indicate that the unconformity is even greater in the deep ocean basins than on the continents and that a transitional sequence will probably never be found, especially in calcareous pelagic sediments. Although disappointing on first inspection, these data never-

theless provide the most tangible and significant clue yet as to the nature of the terminal Cretaceous event.

The late Maastrichtian was a time of general worldwide decrease of temperature with climatic belts becoming more sharply differentiated^{4–9}. It was also a time of greatly reduced clastic influx into the oceans resulting from almost worldwide orogenic quiescence^{1,2,10}. The only major orogeny was the Laramide Revolution in the western interior of North America, the sediments of which apparently never reached marine environments except in the restricted interior sea in which the Lance Group was deposited. It therefore seems likely that the Laramide Revolution, the sediments of which hold the key to terrestrial-marine correlations at the time of the Cretaceous–Tertiary event, was not a significant contributor of carbonate and nutrients to the world's oceans, to which the supply of nutrients and carbonate was then severely restricted^{2,10}.

These sediments do, however, preserve a remarkably complete record of megaphyta and dinosaurs that adds considerably to knowledge of the effects of the Cretaceous–Tertiary event on land. Although the megaphyta are thought to have been little affected by the event, Hall and Norton⁶ described a palynologically significant change across the boundary in the North-Central United States (Laramide sediments) showing rapid replacement of thermophilic dicotyledons by temperate gymnosperms just above the highest dinosaur remains. There is no apparent sedimentary break at the locality. The area is especially significant because the lignite in which the Cretaceous–Tertiary boundary is preserved is laterally equivalent to the lignite just below the base of the fossiliferous marine Cannonball Formation of North Dakota, where there is a nearly continuous Cretaceous–Tertiary sequence^{8,11}. Although calcareous nannofossils are virtually absent in the Cannonball, attesting to its restricted marine environment, rare planktonic foraminifera document its lowest Danian age¹².

In open marine environments, calcareous shelf sections in which rocks of latest Maastrichtian age are known everywhere present similar lithologies. The strata are always rich in glauconite and contain significant amounts of phosphate, both indications of slow deposition². Most of these Upper Maastrichtian marl or chalk beds contain little detritus and many are rich in planktonic foraminifera and nannofossils, an indication that the water column above them was neither hypersaline nor brackish. There are usually hardgrounds or

Fig. 1 Age relationships and present configuration of sediments draped across the mid-Atlantic Ridge at about latitude 30° S, showing the magnitude of the Cretaceous Tertiary Unconformity.

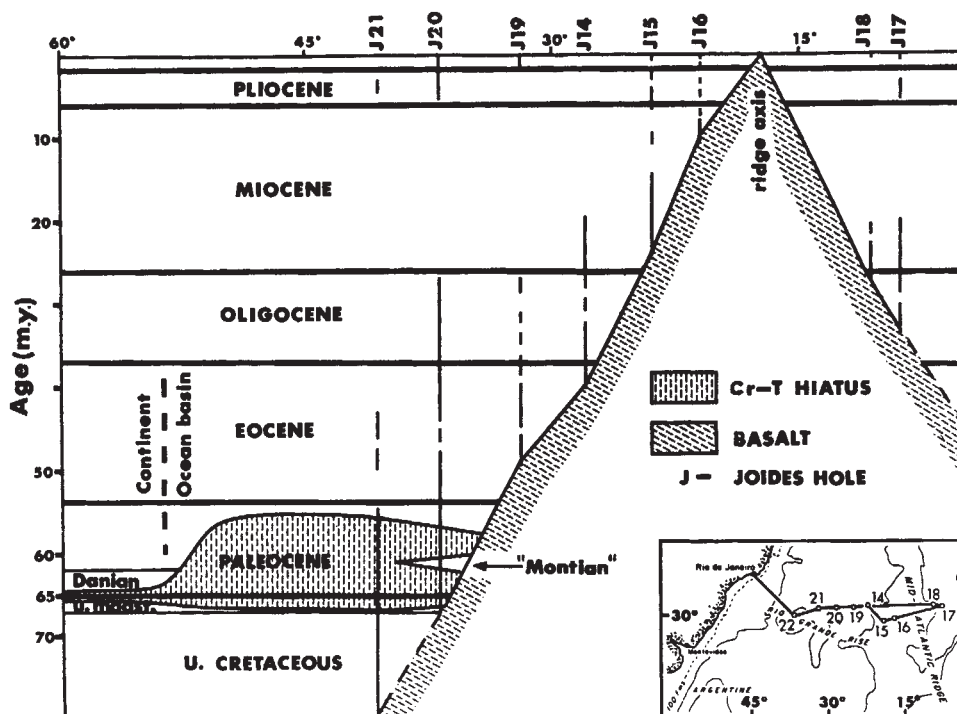
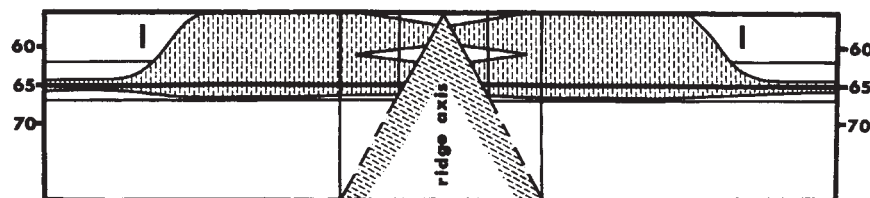


Fig. 2 Late Palaeocene palinspastic reconstruction of Fig. 1. Data are duplicated for the area east of the ridge (where no data exist) for symmetry.



phosphatic-glaucinitic seams containing borings at the top of these strata².

There is good reason to believe that the unusual lithology at the boundary reflects submarine solution of carbonate at the end of the Cretaceous in all oceans; in other words, carbonate compensation depth (CCD) approached or reached the surface of the oceans at this time, as suggested by Tappan². Recent deep sea drilling¹³ substantiates this view by demonstrating a vertical migration of CCD in the Atlantic throughout the Tertiary of more than 1.5 km. Fig. 1 is a diagram intended to show the age relationships and present configuration of sediments draped over the southern part of the Mid-Atlantic Ridge at about latitude 30° S (ref. 14). Information about the Cretaceous-Tertiary boundary has been obtained only from sites J-20 and J-21, so that the Cretaceous-Tertiary unconformity plotted is a schematic lateral extrapolation of the drilling data. The data for the continent are also schematic in that they are not intended to represent South America alone but rather a composite of shelf sections from all continents. If the meagre data for the South Atlantic have been interpreted correctly, there would be a similar cross-section for all ocean basins where there has been seafloor spreading since the late Cretaceous.

Fig. 2 is an Upper Palaeocene palinspastic reconstruction of Fig. 1, which shows that the more continuous deep sea record of the Maastrichtian-Palaeocene interval occurs on the Maastrichtian-Danian Mid-Atlantic Ridge using calculated spreading rates¹⁴. This configuration is reasonable because the shallower ridge areas should have remained above CCD longer than the deep ocean basins but not as long as the shallower marine shelves. The available data suggest that the descent rate of points on the seafloor down the flanks of the Mid-Atlantic Ridge outstripped that of the downward migration of CCD after the Cretaceous-Tertiary event. Thus site J-20, which was almost at the crest of the Ridge in the late Maastrichtian, experienced a pause in calcareous sedimentation as the CCD rapidly migrated toward the surface during the Cretaceous-Tertiary event but received calcareous sediment again shortly after the event as the CCD migrated below the ridge crest. The time during which CCD was above the ridge top represents most of the Danian. Site J-21, which also contains Upper Maastrichtian calcareous sediments, was apparently lower on the ridge flank and never received calcareous sediment until the Upper Palaeocene, when the CCD finally descended low enough to permit calcareous sedimentation in the area of the Rio Grande Rise. The magnitude of the Cretaceous-Tertiary hiatus in the deep sea is therefore a function of palaeobathymetry with deeper water sections exhibiting a greater unconformity.

I will now outline a model of the terminal Cretaceous event in which most of the abrupt faunal and floral extinctions are direct consequences of the late Maastrichtian environmental conditions.

Before the Jurassic, CaCO₃ eroded from the continents was precipitated on the continental shelves by benthonic organisms and only a very small proportion was permanently lost to the deep ocean basins by the agencies of pelagic organisms with calcareous skeletons. With the advent of calcareous plankton in the Jurassic, a mechanism was provided for the removal of carbonate from the orogenic cycle, and the effects were first markedly apparent during the late Cretaceous, when there was no replacement from continents that were nearly base-levelled^{4,10}.

There followed during the late Maastrichtian a pronounced climatic deterioration, and among marine organisms, belemnites, several species of planktonic foraminifera and at least two species of calcareous nannofossils became climatically restricted^{8,9}. Moreover, more rapidly evolving nannofossil taxa were selectively replaced by slowly evolving forms¹⁵. On the continents, cycads and other thermophilic floral elements were being replaced in middle latitudes by temperate conifers and hardwoods, with a concurrent decline of reptiles⁵⁻⁷.

It is suggested that the cause of this deterioration was a decrease of CO₂ in the atmosphere brought about primarily by the late Cretaceous phytoplankton bloom which was responsible for the worldwide deposition of chalk². Other mechanisms for converting CO₂ into O₂ involved the rise of the photosynthetically efficient angiosperms¹⁶ and possibly the removal of carbon from the atmospheric cycle by lignite formation¹⁷. There is no method available for testing directly whether changes in CO₂ content in the atmosphere entail climatic changes, but Plass¹⁸ has calculated that present temperatures would drop about 4° C if the CO₂ content of the atmosphere were reduced by 7%. The same forces would also reduce cloudiness and tend to accentuate the effects of climatic deterioration by making less effective the means of heat transfer between poles and equator, thus making all areas of the globe more dependent on direct insolation. Moreover, lower worldwide temperatures should decrease precipitation and terrestrial weathering and erosion rates, reducing still further the restricted supply of nutrients and carbonate available to marine phytoplankton.

With continuing climatic deterioration in the Maastrichtian, polar cooling of seawater would have increased the horizontal and vertical oceanic thermal gradients³ and this would have increased the solubility of CO₂ in higher latitudes and deeper water. Ultimately, a CCD would have developed in the deep ocean basins and migrated to progressively shallower levels^{2,13}. The absence of early Danian fossils in deep sea cores penetrating the Cretaceous-Tertiary boundary in calcareous sediments is evidence of the process^{13,14,19,20}. Together with the CO₂ increase in the ocean, the lack of detritus reduced the effects of silicate buffering during this time², resulting in a slight lowering of pH. Evidence of this is found in numerous deep sea and shelf phosphate-glaucinite layers and hardgrounds in this part of the column, which suggest submarine solution of carbonate.

The upward migration of the CCD through the late Maastrichtian suggests that the most continuous biological record across the Cretaceous-Tertiary boundary should indeed be found on the shallowest portions of shelves favourable to growth of calcareous nannoplankton. The CCD is supposed eventually to have approached the surface of the ocean and for the first time to have directly affected planktonic organisms^{2,13}, and this is the time of the massive extinctions that ended the Cretaceous. In the marine realm, the sharp phytoplankton reduction would have been associated with extinctions of dependent marine taxa throughout the pelagic food chain. Parenthetically, it is interesting that the sparse nannoflora surviving the terminal Cretaceous event were dominated by *Braarudosphaera* and *Thoracosphaera*, both of which are known to tolerate and even to prefer conditions adverse for the growth of other calcareous nannoplankton²¹.

On land, the latitudinal thermal gradient together with the sharper seasonal and probably diurnal temperature differential readily explains the relatively abrupt extinction of the last

surviving species of dinosaurs⁵. The megaphyta apparently suffered few extinctions across the Cretaceous-Tertiary boundary, but dicotyledons were abruptly shifted toward the Equator in response to fairly rapid chilling of middle latitudes^{6,7}.

The mechanism of recovery from this event is also a part of the model. The removal of a major portion of the phytoplankton would have severely curtailed the photosynthetic conversion of CO₂ to O₂ and of carbonate precipitation in the oceans, finally releasing CO₂ from the oceans into the atmosphere. This increase of atmospheric CO₂ would lead to amelioration of climate and the downward migration of the CCD. The oceans would, however, remain relatively sterile with a somewhat elevated CCD throughout the lower Palaeocene until the supply of nutrients and carbonate was again renewed by orogeny².

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- ¹ Newell, N., *J. Paleontol.*, **36**, 592 (1962).
- ² Tappan, H., *Palaeogeog., Palaeoclimatol., Palaeoecol.*, **4**, 187 (1968).
- ³ Lipps, J., *Evolution*, **24**, 1 (1970).
- ⁴ Lowenstam, H., in *Problems in Paleoclimatology* (edit. by Nairn, A. E. M.), 227 (1963).
- ⁵ Axelrod, D., and Bailey, N., *Evolution*, **22**, 595 (1968).
- ⁶ Hall, J., and Norton, J., *Palaeogeog., Palaeoclimatol., Palaeoecol.*, **3**, 121 (1967).
- ⁷ Stanley, E., *Bull. Amer. Paleont.*, **49**, 179 (1965).
- ⁸ Jeletzky, J., *Twenty-first Geol. Cong.*, part 5, proc. sect. 5, 25 (1960).
- ⁹ Worsley, T., and Martini, E., *Nature*, **225**, 1242 (1970).
- ¹⁰ Bramlette, M., *Science*, **148**, 1969 (1965).
- ¹¹ Fox, S., and Ross, jun., R., *J. Paleontol.*, **16**, 660 (1942).
- ¹² Fox, S., and Olsson, R., *Bull. Amer. Assoc. Petrol. Geol.*, **53**, 734 (1969).
- ¹³ Hay, W., in Bader et al., *Initial Reports of the Deep Sea Drilling Project*, **4**, 672 (1970).
- ¹⁴ Maxwell, A., et al., *Initial Reports of the Deep Sea Drilling Project*, **3** (1970).
- ¹⁵ Worsley, T., thesis, Univ. Illinois (1970).
- ¹⁶ Schindewolf, O., *Neues Jahrb. Geol. Paleontol., Monatsh.*, **10**, 457 (1954).
- ¹⁷ Schwartzbach, M., *Climates of the Past*, 328 (1963).
- ¹⁸ Plass, G., *Tellus*, **8** (1956).
- ¹⁹ Ewing, M., et al., *Initial Reports of the Deep Sea Drilling Project*, **1** (1969).
- ²⁰ Peterson, M., et al., *Initial Reports of the Deep Sea Drilling Project*, **2** (1970).
- ²¹ Hay, W., and Mohler, H., *J. Paleontol.*, **41**, 1505 (1967).

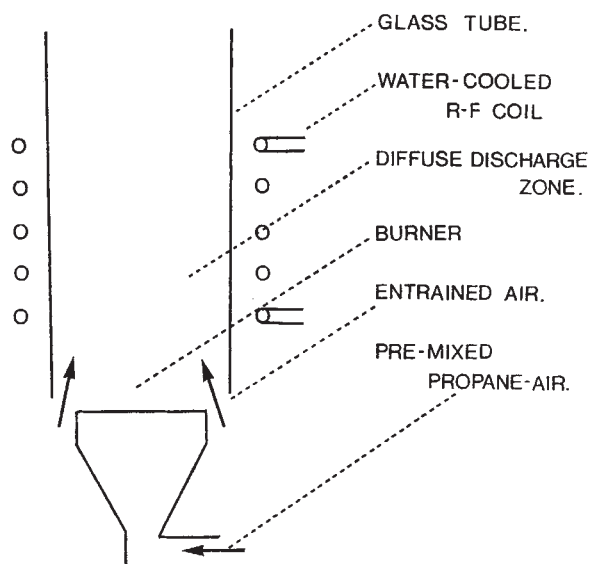


Fig. 1 Augmentation arrangement.

trode energy losses because no electrodes are immersed in the plasma.

The apparatus used is shown in Fig. 1. Currents flowing in closed loop paths in a plane at right angles to the gas flow cause energy to be coupled into the flame; the current generating electric field is induced by the oscillating axial magnetic field of the coil. In our preliminary experiments we have inductively coupled radio frequency power at 6 MHz into a premixed propane/air flame seeded with potassium carbonate. The temperature at 1 kW plate power was found by Na line reversal to rise from 1,900 K at inlet to 2,310 K at outlet, thus demonstrating the feasibility of this form of electrical augmentation. An enhancement of resonance line emission which increased in intensity in the down-stream direction, was observed over the whole cross-section but no visible discharge channel was formed; this showed that there was a relatively diffuse liberation of energy.

For a material of conductivity σ mho m⁻¹ and magnetic permeability μ , the power liberation at a frequency, f , is concentrated principally within the skin depth⁵, δ , where

$$\delta = \text{constant} / \sqrt{f\sigma\mu}$$

(The permeability, μ , is very close to unity in a plasma.)

It has been suggested⁶ that, for efficient coupling, the discharge diameter, D , should obey the criterion

$$2\delta \leq D \leq 10\delta$$

that is, the skin depth can be used as a guide to the size of the discharge.

The variation of skin depth as a function of frequency is shown in Fig. 2 for combustion products of $\frac{3n}{2}$ O₂ and C_nH_{2n}

at one atmosphere pressure⁷ seeded with 0.02 atmospheres of potassium, and for argon at atmospheric pressure seeded with 0.001 atmospheres of caesium. The graphs show the decrease in skin depth caused by the increase in temperature of the combustion products.

The implication of Fig. 2 is that, if the discharge can be prevented from becoming unstable, induction plasmas of very great dimensions can be produced using alkali seeded flames, or other seeded systems (for example, argon seeded with caesium); according to curve 3 in Fig. 2 a plasma 50 cm in diameter could be produced at 10 kHz. The power coupled to the plasma can be controlled by varying the pitch of the coils, so the plasma could, in principle, be made of indefinite length; the power level would have to be just sufficient to make up for losses. It is important from the economic point of view that the frequencies required for plasmas of a size

Flames augmented by Inductive Coupling of Electrical Energy

IN recent years there have been a number of studies of flames augmented by the application of an electrical discharge. The purpose of some has been to increase the final flame temperature, and of others to obtain increased combustion intensity as well as elevated temperatures. Several useful techniques have been developed, each of which has its own particular advantages; these are magnetically rotated arcs¹, a.c. and d.c. arcs applied to turbulent flames^{2,3} (sometimes seeded with easily ionized materials), and a system based on the rapid mixing of a plasma stream with jets of reactants⁴. An earlier attempt to use a radio frequency discharge was unsuccessful¹, because the discharge was very unstable in the presence of fuel. Our work, however, has shown that by seeding the reactants with an easily ionized material radio-frequency coupling can be achieved with certain advantages apart from the obvious ones; we find absence of electrode erosion, no electrode contamination of products and no elec-

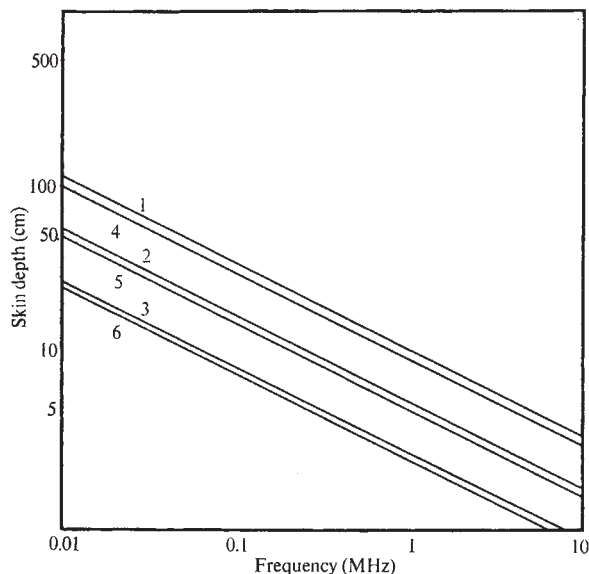


Fig. 2 Variation of skin depth with frequency for various temperatures of seeded combustion products and for argon. 1, 2, 3: Combustion products + 0.02 atm. potassium at 2,600 K, 3,000 K, 4,000 K. 4, 5, 6: Argon + 0.001 atm. caesium at 2,000 K, 2,400 K, 3,500 K.

appropriate for industrial processing rates are within the range of solid state inverters, deuterium thyratrons and rotary generators. Studies of discharge stability and temperature and velocity distributions are in progress.

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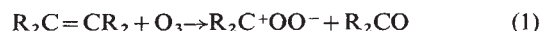
Received October 5, 1970.

- ¹ Chen, D. C., Lawton, J., and Weinberg, F. J., *10th Int. Symp. Combustion*, 743 (The Combustion Institute, 1965).
- ² Karlovitz, B., and Richardson, D. E., *Trans. Amer. Soc. Mech. Eng.*, **61**, 251 (1961).
- ³ Fells, I., Gower, J. C., and Harker, J. H., *Combustion and Flame*, **11**, 309 (1967).
- ⁴ Lawton, J., Payne, K. G., and Weinberg, F. J., *Nature*, **193**, 736 (1962).
- ⁵ Terman, F. E., *Electronics and Radio Eng.* (McGraw-Hill, 1955).
- ⁶ Mironer, A., and Hushfar, F., *A.I.A.A. Tech. Paper 63045* (1963).
- ⁷ Frost, L. S., *J. App. Phys.*, **32**, 10 (1961).

Oxidation of Atmospheric SO₂ by Products of the Ozone-Olefin Reaction

OZONE is the most abundant oxidant in the troposphere and is generated in the stratosphere by the reaction of oxygen atoms, formed by photolysis, with molecular oxygen. Some is then transferred to the troposphere and 0–0.05 p.p.m. of ozone is found in unpolluted air at ground level¹. Ozone can also be formed directly in tropospheric polluted air by the photolysis of NO₂ in the presence of hydrocarbons; concentrations in excess of 0.15 p.p.m. are regularly recorded in Los Angeles² and other cities of the United States. Significant increases in equivalent ozone concentrations over background levels have been recorded in Holland³—up to 0.10 p.p.m. in conditions suitable for the build up of photochemical oxidants. The ambient sulphur dioxide level was found to vary inversely with equivalent ozone level.

The reaction between ozone and SO₂ is too slow to be significant at atmospheric concentrations⁴, but the reaction with unsaturated hydrocarbons is very rapid even at concentrations less than 1 p.p.m. (ref. 5). The products of this reaction include a highly reactive species which has a strong germicidal⁶ and phytotoxic⁷ effect, and it has been suggested that the active species is a peroxide zwitterion formed during the ozone-olefin reaction^{8,9}



Such a species, with a pronounced charge separation, should also be an effective oxidant for the conversion of SO₂ to SO₃.

We report here experiments which demonstrate such an oxidation; they were carried out in a 220 l. aluminium chamber^{10,11} which was flushed out with a stream of purified air before the reactants were introduced. A slow air stream (about 1 l./min⁻¹) also vented the chamber during runs. Relative humidity was 40% in all experiments. Ozone was generated externally by passing 200 cm³ min⁻¹ of cylinder oxygen through a small aluminium box containing mercury vapour ultraviolet lamps. Ozone levels of about 1 p.p.m. were obtained in the chamber after generation for 2 min. The SO₂ was measured into a small glass cell on a vacuum system and injected into the chamber. The reaction was started by injecting the required amount of liquid olefin into the venting air stream, where it vaporized before entering the chamber. A small fan was installed close to the injection point to ensure rapid mixing.

The ozone concentration was monitored continuously using a Brewer cell¹² containing buffered KI solution. The output current was monitored by measuring the potential difference across a 500Ω series resistor. The response time of this system was, however, too slow to record accurately the changing ozone levels in the more rapid reactions. Also SO₂ and oxidant formed during the reaction interfered with the measured values. Hydrocarbon concentrations were measured with a gas chromatograph fitted with a flame ionization detector.

The measurement of SO₂ and its oxidation products using radioactive ³⁵SO₂ has been discussed previously¹⁰. The product of the oxidation, an aerosol, is separated from the gaseous SO₂ on a membrane filter. Both the filters and samples of gaseous SO₂ collected in H₂O₂ solution are assayed by liquid scintillation counting and the mass of sulphur calculated from the count rate.

We first studied the ozone-olefin reaction in the absence of SO₂; the reaction followed second order kinetics and approximately equal amounts of ozone and olefin were consumed. The second order rate constants, *k*₁, for each olefin agreed quite well with previous values⁵.

Large amounts of aerosol were produced when SO₂ was added to the mixture, and because the formation pattern was similar to that produced photochemically^{10,11} we assumed that the aerosol was sulphuric acid. The SO₂ had no significant effect on the rate of disappearance of ozone or hydrocarbon but a small increase in the carbonyl product yields was observed.

Fig. 1 shows the build up of sulphuric acid aerosol (expressed as p.p.m. of SO₃) for four different olefins. In each case the initial reactant concentrations were about 2 p.p.m. olefin, 0.4 p.p.m. ozone, and 0.11 p.p.m. SO₂. There was no observable

Table 1 Variation of the Amount of Aerosol formed by the Ozone-4-methyl-1-pentene Reaction with Initial SO₂ Concentration

Initial SO ₂ concentration (p.p.m.)	Maximum aerosol concentration (p.p.m. SO ₃)	Ratio SO ₃ (max) O ₃ (consumed)	Initial d/dt (SO ₃) (p.p.m./min ⁻¹)
0.058	0.021	0.0525	0.0030
0.108	0.045	0.112	0.0029
1.19	0.060	0.150	0.0030

Initial olefin and ozone concentration was 2.0 and 4.0 p.p.m. respectively.

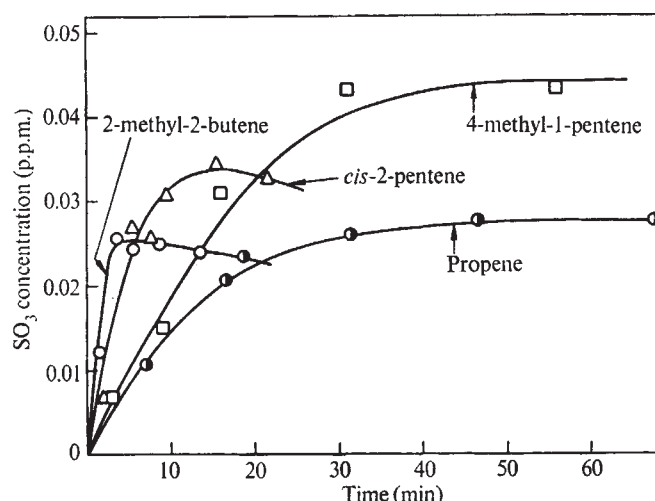


Fig. 1 Sulphuric acid aerosol formation during the oxidation of SO₂ (0.11 p.p.m.) in the presence of O₃ (0.4 p.p.m.) and olefins (2 p.p.m.).

induction period after the addition of olefin to the SO₂-ozone mixture and aerosol formation ceased when the ozone concentration had decreased to zero. The amounts of aerosol formed were similar for all the olefins used and were considerably less than the amount of ozone consumed. Table 1 shows that, for the ozone-4-methyl-1-pentene reaction, the amount of aerosol formed only increased by a factor of 3 when the SO₂ concentration was increased twenty-fold. The initial rate of aerosol production also does not vary with SO₂ concentration. The reaction kinetics are complex and the difference between the amount of ozone consumed and the amount of aerosol formed is not constant during the reaction.

Table 2 Rate of Aerosol Formation for Various Olefins

Olefin	Initial rates (p.p.m. min ⁻¹)		SO ₃ (max) O ₃ (consumed)
	-d/dr (O ₃)	d/dr (SO ₃)	
2-Methyl-2-butene	0.34	> 0.02	0.063
<i>cis</i> -2-Pentene	0.20	0.017	0.085
4-Methyl-1-pentene	0.013	0.0029	0.112
Propene	0.016	0.0021	0.070

Initial concentration was 2.0 p.p.m. olefin, 0.4 p.p.m. ozone and 0.11 p.p.m. SO₂.

Fig. 1 also shows that there is a variation in the initial rates of aerosol production for the different olefins. This variation is expressed numerically in Table 2 which contains data collected in the presence and absence of SO₂. Changes in the initial rate of aerosol formation follow the changes in the rate of disappearance of ozone (determined in the absence of SO₂) quite closely. The rates of aerosol formation are much greater for the internally unsaturated olefins, *cis*-2-pentene and 2-methyl-2-butene, than for the terminally unsaturated propene and 4-methyl-1-pentene.

It is interesting to speculate whether these same oxidants are responsible for the observed photo-oxidation of SO₂ in the presence of oxides of nitrogen and olefins¹¹ because the formation of ozone and aerosol in the latter system shows similar features. Formation of sulphate aerosol does not occur during irradiation of a mixture of NO, olefin and SO₂ until the NO has been converted to NO₂ (ref. 13) and this also applies to the photochemical formation of ozone from nitric oxide and hydrocarbons. Similarly, the maximum aerosol yield coincides with the maximum yields of ozone and peroxyacyl nitrates when the ratio of initial concentrations of olefin to NO₂ is varied in the photolysis of olefin, SO₂ and NO₂ mixtures¹⁴.

The concentration of ozone necessary to account for the photolytic rates of aerosol formation observed in our previous experiments¹¹ can be calculated from d/dr (SO₃) (Table 2). With initial concentrations of 0.33 p.p.m. NO, 0.11 p.p.m. SO₂ and 0.56 p.p.m. *cis*-2-pentene, the maximum rate of aerosol production (5.95×10^{-4} p.p.m. min⁻¹) would require an ozone concentration of 0.112 p.p.m., assuming that the hydrocarbon concentration had dropped to 0.25 p.p.m. when the maximum rate was reached. In the case of propene an ozone concentration of 0.026 p.p.m. would be required to account for the maximum rate of aerosol production (1.37×10^{-5} p.p.m. min⁻¹) with a mixture initially containing 0.4 p.p.m. NO and propene and 0.125 p.p.m. SO₂. Photo-oxidation data for NO-olefin mixtures¹⁵ show that ozone concentrations of this magnitude are certainly produced in this system, and may well account for the formation of sulphuric acid aerosols during photo-oxidation in the presence of SO₂.

The general occurrence in Britain of a germicidal agent in the air has been demonstrated recently^{16,17} and the properties of this so-called open air factor (OAF) are similar to the biologically active products of ozone-olefin reactions. OAF seems to be more intense in rural air than in city air and, although independent of the ozone concentration, is thought to be associated with a reaction between ozone and the olefinic constituents of vehicle exhaust gases¹⁷. The effects of the ozone-olefin reaction seem to be widespread and could make a significant contribution to the oxidation of sulphur dioxide in the atmosphere. At ozone and olefin concentrations of 0.05 p.p.m., the oxidation rate of 0.1 p.p.m. SO₂ is calculated to be about 3% h⁻¹ for *cis*-2-pentene and 0.4% h⁻¹ for propene.

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Received December 29, 1970.

- Junge, C. E., *Air Chemistry and Radioactivity*, 37 (Academic Press, New York and London, 1963).
- Air Quality Data from the National Air Sampling Networks, 1964-1965* (US Department of Health, Education and Welfare, 1966).
- Wisse, J. A., and Velds, C. A., *Atmos. Environ.*, **4**, 79 (1970).
- Cadle, R. D., in *Air Pollution Handbook* (edit. by Magill, P. L., Holden, F. R., and Ackley, C.), 3 (McGraw-Hill, New York, 1956).
- Leighton, P. A., *Photochemistry of Air Pollution*, 159 (Academic Press, New York, 1961).
- Druett, H. A., and Packmann, L. P., *Nature*, **218**, 699 (1968).
- Darley, E. F., Stephens, E. R., Middleton, J. T., and Hanst, P. L., *Intern. J. Air Pollution*, **1**, 155 (1959).
- Dark, F. A., and Nash, T., *J. Hyg., Camb.*, **68**, 245 (1970).
- Arnold, W. N., *Intern. J. Air Pollution*, **2**, 167 (1959).
- Cox, R. A., and Penkett, S. A., *Atmos. Environ.*, **4**, 425 (1970).
- Cox, R. A., and Penkett, S. A., *Nature*, **229**, 486 (1971).
- Brewer, A. W., and Milford, J. R., *Proc. Roy. Soc., A*, **256**, 470 (1960).
- Wilson, jun., W. E., and Levy, A., *J. Air Poll. Control Assoc.*, **20**, 385 (1970).
- Altshuller, A. P., and Bufalini, J. J., *Photochem. Photobiol.*, **4**, 97 (1965).
- Altshuller, A. P., Kopczynski, S. L., Lonneman, W. A., Becker, T. L., and Slater, R., *Environ. Sci. Tech.*, **1**, 899 (1967).
- Druett, H. A., and May, K. R., *Nature*, **220**, 395 (1968).
- Druett, H. A., and May, K. R., *New Scientist*, March 13 (1969).

Electric Effects of the Failure of Fibre Reinforced Plastics

WE report some preliminary results of a study of the electrical effects associated with the deformation and failure of fibre reinforced plastics (epoxy resins). It has been previously reported¹ that the current generated by a small voltage, applied across the thickness of the gauge section of a tensile specimen

made from such a composite, changes during straining. We have found that local breakdown due to fibre debonding and pull-out can cause such current changes.

Mahieu has demonstrated¹ that there are electrical and luminescent effects associated with the failure of glass reinforced resins. He showed that current changes of between 10^{-12} and 10^{-10} A occurred both in the direction of the bias current and/or in opposition to it when a tensile specimen was loaded to failure.

Our circuit is similar to Mahieu's and two overlapping contacts were made to opposite sides of the specimen with conducting paint. The current was 3×10^{-12} A and was detected by an electrometer with a maximum sensitivity of 10^{-14} A. All the specimens were fabricated from a plasticized epoxy resin (Ciba Ltd, 60 parts MY 750, 40 parts CY 208 and 10 parts HY 951). The specimens were strained at a rate of 1 mm min^{-1} ; the tensometer and electrometer head amplifier were enclosed in an earthed brass gauze to screen out extraneous

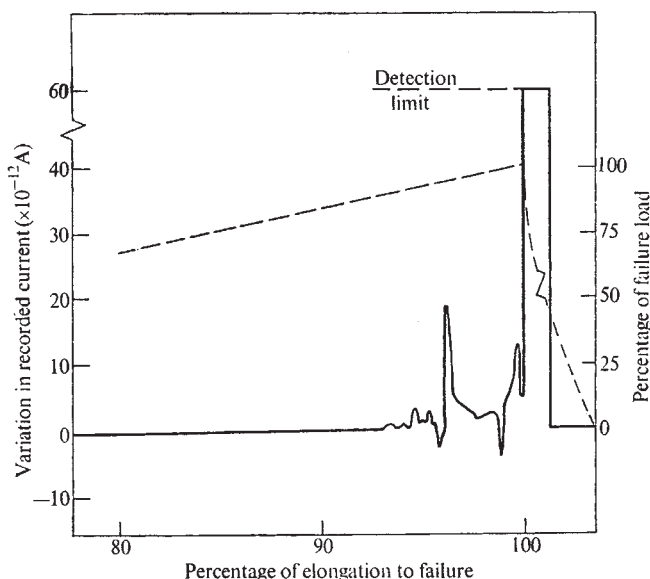


Fig. 1 Variation in recorded current and applied load for a cross-plyed $0+90^\circ$ glass fibre reinforced epoxy resin. The applied current level is 3×10^{-12} A.

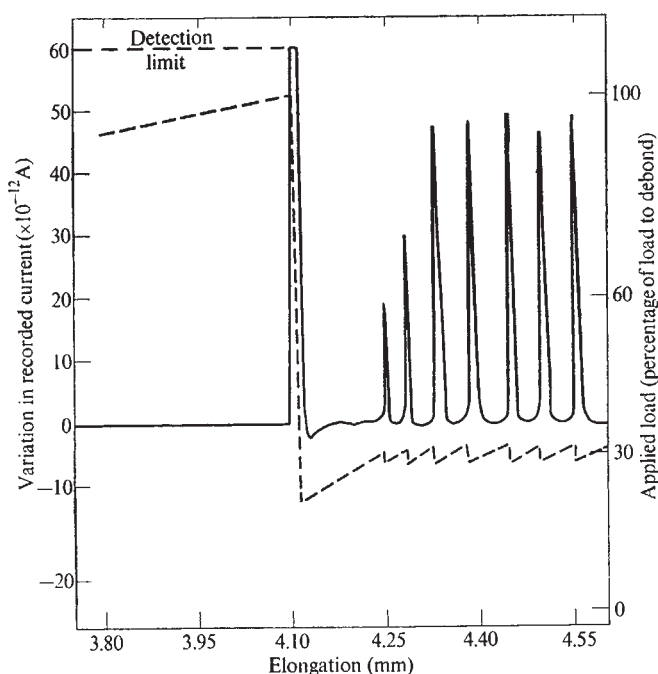


Fig. 2 Variation in recorded current and applied load for a model system of one reinforcing filament in epoxy resin. The applied current level is 3×10^{-12} A.

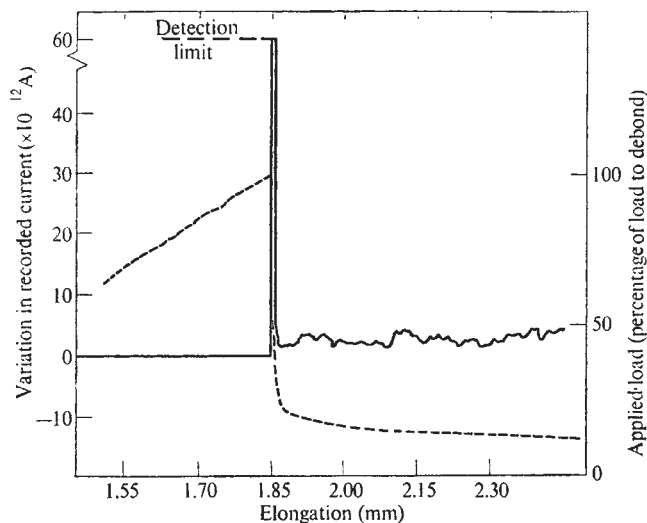


Fig. 3 Variation in recorded current and applied load for a model system of one P.T.F.E. coated reinforcing filament in epoxy resin. The applied current level is 3×10^{-12} A.

electrical fields. The output from the load cell and the electrometer was recorded on a two pen chart recorder.

We detected current changes of the same order as Mahieu in cross-plyed laminates of E glass and epoxy, the larger changes occurring simultaneously with loud audible clicks (Fig. 1). A series of specimens consisting of a single unidirectional layer was tensile tested along the fibre direction to eliminate the effects of cross-plying. The changes in the measured current were found to begin in most specimens at 90% of the failure load (as low as 60% in one test) and to be generally of a lower value than observed with multilayer cross-plyed specimens. Most of the specimens showed an appreciable amount of fibre pull-out in the fracture surface but in the case of a very smooth fracture (little fibre pull-out) there was no detectable current change before final failure occurred.

Separate experiments were used to study the effect of fibre debonding and pull-out in order to determine whether these current changes could be correlated with distinct failure mechanisms. A model system consisting of a single $550 \mu\text{m}$ diameter cold drawn phosphor-bronze wire embedded in epoxy resin was used. A plane of weakness was incorporated so that the failure of the matrix on a flat plane perpendicular to the filament axis occurred at a low load and the filament then carried all the load.

A load-elongation curve and the associated curve showing the variation in recorded current with the reinforcing filament as one of the electrodes is shown in Fig. 2; the other electrode was again a side of the specimen. The load increases approximately linearly until at a critical value, the fibre debonds from the matrix (G. D. S., unpublished results). There is a large current pulse associated with this debonding which is cut off by the limit of the electrometer range, and also a loud audible click. After further straining the fibre pulled out of the matrix in a manner consistent with a stick-slip mechanism. Current pulses of approximately 5×10^{-11} A and audible clicks were detected, and these could be correlated with the corresponding drop in load on the stick-slip portion of the load-elongation curve.

A double beam oscilloscope showed that the drop in load and the associated current pulse occurred within 5 ms of each other. This difference could easily be a consequence of time delays in the electrometer or load cell circuitry.

Specimens were fabricated using P.T.F.E. coated phosphor-bronze wires to reduce the amplitude of the stick-slip mechanism; the results are shown in Fig. 3. The load curve decreases smoothly after the fibre debonds, with a small continuously varying change in the recorded current (less than 5×10^{-12} A).

These changes did not consist of discrete pulses decaying rapidly to the bias current, in contrast to Fig. 2.

Effects due to the matrix material alone were eliminated by testing a series of specimens of unreinforced epoxy of the same composition as before. No changes in the recorded current were detected before final failure.

These variations in recorded current level could form the basis of a quality control test. The particular advantage would be that the defects could be determined when a component was stressed at any value up to and including the design or proof load. There is no test damage additional to that done by the service load itself and the test is completely non-destructive if no defects are initiated or propagated by the test load. In-service monitoring might also be possible.

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¹ Mahieu, W., *Electrical and Luminescent Effects of Fracture in Some Fibre Reinforced Plastics*, Air Force Materials Laboratory (Wright-Patterson Air Force Base) TR-68-113 (1968).

BIOLOGICAL SCIENCES

Cranium of the Late Palaeocene Primate *Plesiadapis tricuspidens*

ALTHOUGH Palaeocene primate crania other than that of *Plesiadapis tricuspidens* are known (R. W. Wilson and I are now describing a crushed skull of *Palaechthon alticus* R. W. W.), the slightly crushed and damaged skull of the French primate (CR 125), described by Russell^{1,2}, is still the only near complete cranium of a Palaeocene primate. As well as Russell's descriptions there have been three reconstructions of this specimen, two by Simons^{3,4} and one by Russell². My remarks here will be directed to Simons's 1964 synthesis.

Through the courtesies of Dr J.-P. Lehman and Dr D. E. Russell, both of Muséum d'Histoire Naturelle, I have re-examined CR 125. My findings are incorporated into a new reconstruction of the lateral view of the skull and mandible of *Plesiadapis tricuspidens*, prepared by Miss Daria Dykij under my direction. My remarks pertain chiefly to the differences of the reconstruction presented here from those of the previous authors.

The major discrepancy between the past and present syntheses lies in the interpretation of the splanchnocranium and the occlusal relationship of the enlarged incisors. The previous figures show (1) the muzzle slightly bent dorsally to the plane of the neurocranium, and (2) the tip of the enlarged lower incisor occluding in between the space of the two mesial cusps of the enlarged upper incisor. In addition to showing the incisors in active occlusion, the cheek teeth are also illustrated in tight occlusal contact.

A brief survey of the occlusal relationship of teeth in mammals with enlarged incisors followed by a diastema reveals that when the cheek teeth are in occlusal contact the tips of the enlarged lower incisors are dorsal and posterior to the tips of the enlarged upper incisors. Conversely, whenever the lower incisor tips make occlusal contact with the apices of the cusps of enlarged upper incisors the cheek teeth become widely separated. This dichotomy exists merely to coordinate two separate masticatory functions, nipping and gnawing on one hand by the incisors, and shearing and crushing on the other by the cheek teeth.

The earlier reconstruction of the anterior region of the skull and of the occlusal relationships may have been faulty because the mandible was fitted with a cranium which had a diastema between the enlarged incisors and the premolars having different proportions from that of CR 125. All the diastemas of the mandibles of *P. tricuspidens* from Cernay vary in their dimensions and in their relation to the length of the cheek tooth row and the orientation of the lower incisor. There is no reason to doubt that this variation extended to the crania of these respective mandibles. Mandibles CR 424 and CR 402 show clearly the kind of individual variation in the length of the diastema and the orientation of the incisors within the species.

Thus an attempt to occlude the cheek teeth of one of the best preserved mandibles, CR 204 (a younger individual with a relatively long diastema for the sample), with the skull would invariably result, as Russell and Simons figured it, in simultaneous occlusal contact of the enlarged incisors and the cheek teeth. The large and robust mandible occluded with CR 125 is probably not of the same proportion as the mandible which belonged with the skull. To compensate for the great length of the lower diastema and keep the incisor tips in occlusion while the cheek teeth occluded—a mistaken *a priori* assumption—both Simons and Russell showed the muzzle of CR 125 as bent dorsally. But neither the slightly crushed skull nor any theoretical considerations justify the position of the muzzle as previously shown.

Another difference of opinion concerns the relative size and contact of the enlarged premaxillaries with the frontals. Russell³ (page 88) thought that both premaxillaries were crushed identically dorsally, and cites the direction of the premaxillary-maxillary suture as indicative of posterior elongation of the premaxillaries, which in his view were in contact with the frontals. I believe that this interpretation is incorrect and that the downward crushing of the nasals resulted in the medial and slightly posterior folding of the premaxillaries. I do not think it probable that nearly as much was broken off the premaxillaries as indicated by Russell's Fig. 14, and thus the premaxilla-frontal contact was unlikely. Fig. 1 of this communication (supported by Fig. 2) indicates the extent to which, in my interpretation, the premaxillary extended posteriorly.

Certain features of the orbital region of *Plesiadapis* and other early Tertiary primate crania necessitate comment. One of the supposedly diagnostic features of primates is the presence of the ethmoid component in the orbit, the "os planum", as Martin has recently⁷ said. Because the "os planum" is entirely absent from the orbit of *Plesiadapis*³, Eocene adapids (*Notharctus*, *Smilodectes*, *Adapis*, *Pronycticebus*) as well as *Necrolemur*, including the presence of this bone in the orbit in a "definition" of the order is inexplicable. There seems to be little doubt that the absence of the ethmoid from the orbital region is indeed primitive in the order, and its occurrence in *Tarsius*, lorisoids, most lemurs (except

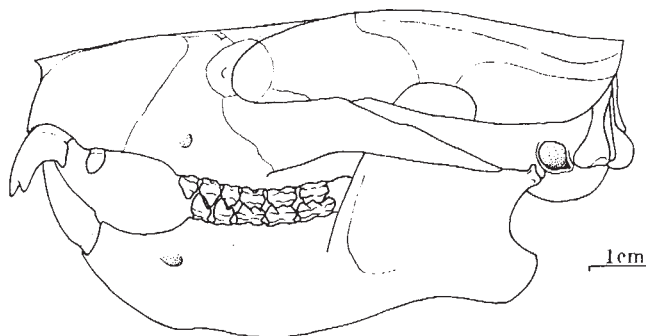


Fig. 1 Reconstruction of lateral view of the cranium and mandible of *Plesiadapis tricuspidens*, based chiefly on CR 125 (skull) and CR 402 and 424 (mandibles); late Palaeocene, France.

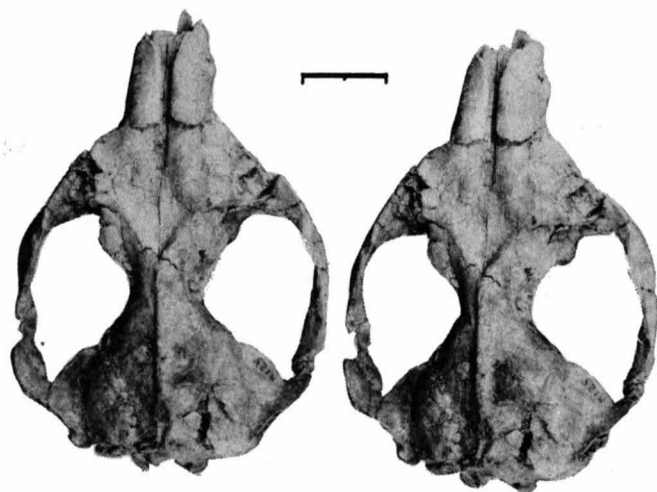


Fig. 2 Cranium of *Plesiadapis tricuspidens* (CR 125), dorsal view, stereoscopic pairs. The bar represents 2 cm.

Daubentonia), platyrrhines and catarrhines is clearly an advanced feature, probably attained independently. (Simons and Russell did not agree about lemurs, maintaining that "... only in Cheirogaleinae, among living and fossil Lemuriformes, is the os planum present". The evidence, however, has been clearly presented in ref. 6.

An unusual feature of the *Plesiadapis* cranium, compared with primitive placentals as well as other early Tertiary primates, such as *Phenacolemur*, adapids, or microchoerids, is the extreme posterior position of the glenoid fossa. The function of this posterior displacement of the craniomandibular articulation is unknown; its cause is probably best sought in the mandibular mechanics of the genus.

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¹ Russell, D. E. *Bull. Soc. Geol. France*, **7**, 1 (1959).

² Russell, D. E., *Mem. Mus. Nat. Hist. Nat.*, **C**, 13 (1964).

³ Simons, E. L., *Amer. Sci.*, **48**, 2 (1960).

⁴ Simons, E. L., *Sci. Amer.*, 211 (1964).

⁵ Simons, E. L., and Russell, D. E., *Breviora*, 127 (1960).

⁶ Forsyth Major, C. I., *Proc. Zool. Soc. London*, **1**, 9 (1901).

⁷ Martin, R. D., *Man*, **3**, 3 (1968).

DDT metabolized by Microorganisms from Lake Michigan

PERSISTENT chlorinated hydrocarbon insecticides have been used throughout the world for the past two decades, and their residues or metabolites are accumulating in soil and water. There are several reports of microorganisms converting 1,1,1-trichloro-2,2-bis(p-chlorophenyl) ethane (DDT) to 1,1-dichloro-bis(p-chlorophenyl) ethane (TDE)¹⁻⁷. Other reports indicate that TDE is the principal product of DDT metabolism in aquatic environments^{8,9}. There are, however, no reports of the metabolism of DDT by aquatic microorganisms in pure culture. Here we describe a study of the metabolism of DDT by microorganisms isolated from water and bottom silt of Lake Michigan and related water systems.

Twenty-four samples from water and bottom silts (upper and lower strata) were collected from different ecological situations from the Kenosha, Sturgeon Bay and Green Bay

areas in Wisconsin. Both the top layer silt sample and the core samples (6-12 inches deep) were collected using a borer 2 inches in diameter. Isolation and culture techniques used were similar to those described before¹⁰. To measure insecticide incorporation, a 72 h culture in 10 ml. of yeast extract-mannitol broth¹¹ was incubated with 10^{-5} mol ¹⁴C-labelled DDT—added to the medium in 10 μ l. of ethanol—in a screw capped 20 ml. test tube at 30° C for 30 days without shaking. Extraction and other procedures were similar to those described before¹⁰.

Table 1 shows the numbers of microorganisms isolated from the water, silt and soil samples. The majority of approximately 300 isolates from water and soil could metabolize DDT. The principal metabolite found was TDE. A number of isolates forming TDE from DDT also produced a dicofol-like compound. To identify this commonly occurring metabolite of DDT, an authentic sample of 1-bis(p-chlorophenyl)-ethane (commonly known as DDNS) was prepared by a dechlorination reaction from DDT by using zinc powder and HCl in acetic acid. The chemical identity of this reference sample was confirmed by nuclear magnetic resonance and mass spectroscopy. Thin-layer chromatographic analysis of the microbial and reference samples, with three different solvent systems (solvent mixtures of n-pentane 15 and ethyl acetate 1; acetone 1 and hexane 4; hexane 10, ethanol 2 and acetic acid 1) showed that the dicofol-like compound produced by microorganisms matched the reference compound in all tests, the Rf-value of DDNS in these chromatographic systems being 0.32, 0.35 and 0.42 respectively.

The results in Table 1 also indicate that the number of isolates obtained per water or silt sample does not vary significantly between the samples from Lake Michigan (representing relatively clean aquatic environments) and those from its tributaries (representing the area of higher eutrophication);

Table 1 Metabolic Activity of Microbial Isolates from Lake Michigan and its Tributaries to degrade DDT

	No. of cultures isolated (No. of samples)	No. of cultures forming TDE (% of total isolates)	No. of cultures forming DDNS (% of total isolates)	No. of cultures forming DDE (% of total isolates)
Lake Michigan				
Water	41 (5)	36 (88)	18 (44)	9 (22)
Top silt *	45 (4)	34 (76)	20 (44)	7 (16)
Bottom silt †	57 (3)	45 (79)	24 (42)	13 (23)
Tributaries I				
Green Bay				
Water	26 (3)	22 (85)	14 (54)	5 (19)
Top silt	28 (2)	21 (75)	14 (50)	12 (43)
Bottom silt	22 (1)	17 (77)	11 (50)	7 (32)
Tributaries II				
River ‡				
Water	11 (1)	11 (100)	5 (45)	3 (27)
Top silt	14 (1)	12 (86)	13 (93)	10 (72)
Tributaries III				
Shoreline§				
Water	31 (2)	21 (68)	15 (48)	7 (23)
Top silt	17 (1)	14 (82)	10 (59)	8 (47)
Bottom silt	13 (1)	10 (77)	6 (46)	6 (46)
Average of all isolates from tributaries				
Water	68 (6)	54 (79)	34 (50)	15 (22)
Top silt	59 (4)	47 (80)	37 (63)	30 (51)
Bottom silt	35 (2)	27 (77)	17 (49)	13 (37)

* "Top silt" represents the uppermost surface layer of the lake or river bottom.

† "Bottom silt" indicates the portion of lake bottom silt between 6 and 12 inches deep, depending upon the depth of the core dug.

‡ Fox River, approximately 2 miles upstream of the Green Bay junction.

§ Both samples are taken from boat loading area representing the near shore stagnant water.

the number of isolates per sample for water samples being 8.2 for Lake Michigan and 11.3 for all other tributaries.

As for the metabolic activities of those isolates, there was a general tendency for the number of TDE forming microorganisms to be constant; there were more DDNS forming microbes in the water and silt samples from the tributaries than in the isolates from Lake Michigan; in comparison with the silt samples from Lake Michigan, those from the tributaries were more capable of converting DDT to DDE.

In an attempt to determine the metabolic routes involved in producing DDNS, an important metabolic product of DDT, ^{14}C -TDE was incubated with the active isolates. The results indicated that DDNS formation from TDE in those microbes was the chief metabolic pathway. It is likely, therefore, that the microorganisms first dechlorinate DDT to form TDE and then TDE is dechlorinated in the same way that DDT is dechlorinated to form TDE. In fact, in many isolates the spots corresponding to TDE were also found to accompany the spot representing DDNS. Both DDNS and TDE are highly acricidal compounds. Their toxic effects on aquatic fauna and flora need to be studied and the residues of both metabolites in aquatic environments should be monitored.

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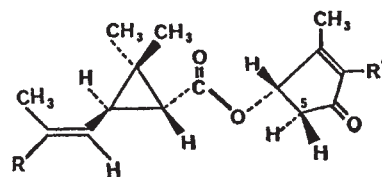
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Received July 29, 1970.

- ¹ Johnson, B. T., Goodman, R. N., and Boldberg, H. S., *Science*, **157**, 560 (1967).
- ² Kallman, B. J., and Andrews, A. K., *Science*, **141**, 1050 (1963).
- ³ Mendel, J., and Walton, M., *Science*, **151**, 1527 (1966).
- ⁴ Wedermeyer, G., *Science*, **152**, 647 (1966).
- ⁵ Guenz, W. D., and Beard, W. E., *Science*, **154**, 893 (1967).
- ⁶ Ko, W. H., and Lockwood, J. L., *Canad. J. Microbiol.*, **14**, 1069 (1968).
- ⁷ Patil, K. C., Matsumura, F., and Boush, G. M., *Appl. Microbiol.*, **19**, 879 (1970).
- ⁸ Miskus, R. P., Blair, D. P., and Casida, J. E., *J. Agric. Food Chem.*, **13**, 481 (1965).
- ⁹ Finley, R. B., and Pillmore, R. E., *Amer. Inst. Biol. Sci. Bull.*, **13**, 41 (1963).
- ¹⁰ Matsumura, F., and Boush, G. M., *Science*, **156**, 959 (1967).
- ¹¹ Fred, E. B., and Waksman, S. A., *Laboratory Manual of General Microbiology* (McGraw-Hill, New York, 1928).

Oxidative Metabolism of Pyrethrins in Mammals

THE natural pyrethrins present in pyrethrum flowers (*Chrysanthemum cinerariaefolium*) have attracted attention for more than a century because they are powerful insecticides yet have very low toxicity to mammals^{1,2}. Concern about the persistence of some insecticides in mammals and in the environment has stimulated a consideration of the degradability of widely used compounds, both old and new. The two principal insecticidal constituents of pyrethrum, pyrethrins I and II (Fig. 1), do not persist in the environment because they are unstable when exposed to light and air³. There is some information about the metabolism of pyrethrin I and related compounds in houseflies^{4,5}, but little is known of the fate of pyrethrum constituents in mammals^{6,7} even though man is often exposed to pyrethrins from household aerosols and other sources. We therefore examined the degradation of pyrethrin I and of pyrethrin II in rats fed separately with large amounts of pure tritium-labelled pyrethrins I and II, prepared with high specific activities by a new procedure involving a direct exchange reaction on (+)-



Designation	R	R'
Pyrethrins		
Pyrethrin I	CH ₃	-CH ₂ -CH=CH-CH=CH ₂
Pyrethrin II	CO ₂ CH ₃	-CH ₂ -CH=CH-CH=CH ₂
Pyrethrins met. A	CO ₂ H	-CH ₂ -CH=CH-CHOH-CH ₂ OH
Pyrethrins met. B	CO ₂ H	-CH ₂ -CHOH-CH=CH-CH ₂ OH
Pyrethrins met. C	CO ₂ H	-CH ₂ -CH=CH-CH-O-conj. CH ₂ OH
Pyrethrins met. D	CO ₂ H	-CH ₂ -CH=CH-CH=CH ₂
Allethrin		
Allethrin	CH ₃	-CH ₂ CH=CH ₂
Allethrin met. a	CO ₂ H	-CH ₂ -CHOH-CH ₂ OH
Allethrin met. b	CO ₂ H	-CHOH-CH=CH ₂

Fig. 1 Structures of pyrethrin I, pyrethrin II, allethrin and of compounds found as their metabolites in rats.

pyrethrolone⁸. The metabolites were isolated in milligram quantities from urine; nuclear magnetic resonance (NMR) and mass spectrometry (MS) showed that each of the principal metabolites retains the cyclopropane ester linkage and is formed by modification of both the acid and alcohol moieties. The oxidation of the acid side chain is similar in type to that encountered previously in other systems^{4,5}, but the establishment of attack on the alcohol part is new and involves the unsaturated side chain as detailed below.

Four samples of labelled pyrethrins, stereochemically and radiochemically pure, were prepared especially for this study: pyrethrins I and II each labelled with ^3H in the methyl group and in the 5-methylene group of the cyclopentenolone ring⁸; pyrethrin I labelled with ^{14}C in the carboxyl group of the acid⁹; pyrethrin II labelled with ^{14}C in the methyl of the methoxycarbonyl group. Important features of the labelled materials were their stereochemical purity and very high specific activity (~ 0.5 Ci/mmol) which facilitated detection and estimation by scintillation counting and autoradiography. Appropriate mixtures of ^3H -labelled and unlabelled pyrethrins^{8,10-12} in dimethyl sulphoxide were given by stomach tube to male rats weighing 180-210 g. The low acute toxicity of the pyrethrins was evident, for the rats survived total doses of 450-2,000 mg/kg pyrethrin I and >2,900 mg/kg pyrethrin II administered in 2-6 portions within 12-54 h; acute oral LD₅₀ values for single doses were 260-420 and >600 mg/kg, for pyrethrins I and II, respectively.

Metabolites in the urine of rats receiving 1.2 g of pyrethrin I or 2.4 g of pyrethrin II were isolated by adding ammonium sulphate and ethanol followed by extraction with ether¹³. The ether-ethanol soluble products were separated and purified without structural change by chromatography, initially on silicic acid-'Celite' columns developed with hexane-ether then acetone-methanol mixtures; after methylation (diazomethane) the methyl esters of the metabolites were rechromatographed (^3H monitoring) and obtained in high purity as estimated by ultraviolet visualization and by autoradiography respectively, on thin-layer chromatograms [silica gel F₂₅₄; chloroform-methanol (39:1), benzene saturated with formic acid-ether (10:3) or ether-benzene-methanol-formic acid (173:12:11:4)]. Both the high specific activities of the products and the NMR spectra indicated that the fractions were substantially pure. The structures of individual metabolites were then assigned by NMR spectroscopy¹⁴ with spectrum accumulation and usually by MS as well, after trimethylsilylation of hydroxyl groups.

The principal metabolite of the pyrethrins excreted in rat urine has structure *A* (Fig. 1). The NMR spectrum indicated that the structure was modified only at R and R'. R was CO₂H because the metabolite after methylation contained the *trans*-methoxycarbonyl group [R=CO₂CH₃] as the only alteration on the acid part of the molecule whereas R' was *cis*-4,5-dihydroxypent-2-en-1-yl based on both NMR and MS. Another metabolite, *B*, is similar but both NMR and MS showed that its alcohol side chain, R', is *trans*-2,5-dihydroxypent-3-en-1-yl (Fig. 1). Both metabolites *A* and *B* are formed in rats from pyrethrin I and from pyrethrin II by oxidation of the *trans*-methyl group and hydrolysis of the methoxycarbonyl group, respectively; each pyrethrin is further attacked on the pentadienyl side chain, possibly to give initially a 4,5-epoxide from which the two diols are derived. The structure of a third metabolite, *C*, of both pyrethrins is indicated by NMR spectroscopy to be a conjugate of metabolite *A*, with the 4-hydroxy group of the diol side chain esterified with an unidentified aromatic acid. The identified metabolites from pyrethrin II lack the methoxycarbonyl group; ¹⁴C from ¹⁴CH₃O-labelled pyrethrin II did indeed appear largely (53%) as ¹⁴CO₂ expired by treated animals. Thus, although the CO₂CH₃ group is hydrolysed quickly, the cyclopropane carboxylic ester group is cleaved only to a minor extent, if at all, because the labelled metabolites detected in the urine were the same whether the rats were fed pyrethrin I labelled with ³H-alcohol or ¹⁴C-acid. Other more polar metabolites in the urine remain to be characterized.

Each metabolite found in the urine was also present in the faeces. Moreover, the faeces but not the urine contained some unmetabolized compound, more of pyrethrin I than of pyrethrin II.

From 64% to 71% of the ³H in the administered dose of both compounds was recovered in the excreta within 100 h and was distributed almost equally between the urine and faeces. The excreted compounds and their content of ³H relative to the administered radioactivity were: 14–21% as metabolite *A*, 3.3–4.4% as metabolite *B*, 3.9–6.2% as metabolite *C*, and 4–18% as unmetabolized compounds, with the remainder as unidentified compounds, mostly of greater polarity. About the same proportions of metabolites were obtained whatever the dose in the range of 0.1–400 mg/kg, so the oxidative mechanisms in the mammal seem not to be overloaded even at the higher dose.

Oxidations of the types found here for the pyrethrins are usually initiated by the microsomal mixed-function oxidase (MFO) system fortified with NADPH^{4,5,15,16}. Pyrethrin I is readily metabolized in the MFO system derived from housefly abdomens at the *trans*-methyl group on the acid side chain, yielding metabolite *D*^{4,5}, but here no significant amounts of this product were detected in the excreta of rats treated with pyrethrins I and II. Since even small amounts of rat liver homogenate in the presence or absence of NADPH hydrolysed pyrethrin II to metabolite *D*, this is possibly a transient intermediate in the *in vivo* metabolism by rats, which then produce more extensive degradation.

With allethrin (Fig. 1) two metabolites (*a* and *b*) were identified. As for pyrethrin I, the sites of attack were the *trans*-methyl on the side chain of the acid and the double bond of the alcohol side chain, but with allethrin there was also hydroxylation at the methylene group of the allyl side chain and hydrolysis of the central ester linkage because chrysanthemum dicarboxylic acid and allethrolone were present in the urine.

The ease of oxidative metabolism of pyrethrins and allethrin possibly contributes to, or accounts for, their low toxicity to mammals.

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Received July 31; revised October 8, 1970.

- ¹ Crombie, L., and Elliott, M., *Fortschr. Chem. Org. Naturstoffe*, **19**, 120 (1961).
- ² Metcalf, R. L., *Organic Insecticides, Their Chemistry and Mode of Action* (Interscience, New York, 1955).
- ³ Chen, Y.-L., and Casida, J. E., *J. Agric. Food Chem.*, **17**, 208 (1969).
- ⁴ Yamamoto, I., and Casida, J. E., *J. Econ. Entomol.*, **59**, 1542 (1966).
- ⁵ Yamamoto, I., Kimmel, E. C., and Casida, J. E., *J. Agric. Food Chem.*, **17**, 1227 (1969).
- ⁶ Audiffren, M., *J. Pharmacie et Chimie*, **19**, 535 (1934).
- ⁷ Menzie, C. M., *Metabolism of Pesticides*; Bur. Sport Fisheries and Wildlife, Special Scientific Report—Wildlife No. 127 (Washington).
- ⁸ Elliott, M., Kimmel, E. C., and Casida, J. E., *Pyrethrum Post*, **10**, (2), 3 (1969).
- ⁹ Yamamoto, I., and Casida, J. E., *Agric. Biol. Chem. (Tokyo)*, **32**, 1382 (1968).
- ¹⁰ Elliott, M., *J. Chem. Soc.*, 5225 (1964).
- ¹¹ Maciver, D. R., *Pyrethrum Post*, **9** (4), 41 (1968).
- ¹² Elliott, M., and Janes, N. F., *Chem. and Ind.*, 270 (1969).
- ¹³ Slade, M., and Casida, J. E., *J. Agric. Food Chem.*, **18**, 467 (1970).
- ¹⁴ Bramwell, A. F., Crombie, L., Hemesley, P., Pattenden, G., Elliott, M., and Janes, N. F., *Tetrahedron*, **25**, 1727 (1969).
- ¹⁵ *Enzymatic Oxidation of Toxicants* (edit. by Hodgson, E.) North Carolina State University at Raleigh, 1968).
- ¹⁶ Parke, D. V., *The Biochemistry of Foreign Compounds* (Pergamon, London, 1968).

Quasi-Energy—a New Bioenergetic Quantity

IN a mathematical study of the concept of the energetically optimal body weight¹ of an animal, a new bioenergetic quantity appeared which we called "quasi-energy"². It is defined as the total heat produced metabolically by the body, *Q*, multiplied by the specific heat production, *q*, the heat production per unit weight of the body. Thus, if *F* is the quasi-energy and *Q* = *q**w*, *w* being the body weight,

$$F = wq^2 \quad (1)$$

This quantity is of interest because its variation with body weight resembles a standard potential energy curve in that there are "potential energy wells" and "potential energy barriers" and because data from experiments with shrews have suggested a connexion between specific points on the quasi-energy curve and crucial stages in the life cycle of the animal. The wells and barriers, for example, seem to coincide with the survival period in winter and the reproductive period in spring³. Here, by considering the dimensions of quasi-energy and the structure of the equations which define it, we clarify some of its physical characteristics and the way in which it is related to physical energy.

The dimensions of *F* can be expressed in SI units using equation (1): if *q* = *Q*/*w*

$$[F] = [Q]^2[w]^{-2} \quad (2)$$

and the dimensions are cal² s⁻² N⁻¹, where N is the newton, the SI force unit defined as 1 kg m s⁻¹. More light can be shed on

the nature of quasi-energy by comparing equation (2) with equation (3) that expresses energy in terms of acceleration and the joule (J):

$$E = \frac{1}{2}mv^2 \quad (3)$$

where m is the body mass (kg), v is the mechanical velocity of the body (m s^{-1}) and J , in SI units, is equivalent to Nm. The two equations are connected because $w = mg$, g being the gravitational acceleration constant, and because Q and v share the same dimensions, m s^{-1} . This becomes more obvious when one transforms the SI heat flow measurement (cal s^{-1}) into the SI power unit, the watt (W):

$$Q \text{ cal s}^{-1} \approx 4.2 Q \text{ W}$$

and, because $Q = qw$, $[4.2q] = \text{Nm/Ns}$
 $= \text{m s}^{-1}$

Thus the terms of equations (2) and (3) have similar dimensions and F can be expressed by the formula

$$F = g[m(4.2q)^2]$$

in which the square brackets enclose the energy term. Therefore,

$$[F] = [g][E]$$

and the new bioenergetic quantity is related to physical energy simply through the gravitational constant.

One peculiarity of quasi-energy is that only in the steady state does it conform with the conservation law,

$$Q = Q_1 + Q_2 \quad (4)$$

in which Q is the input (respiratory) power, Q_1 is the excess (growth) power and Q_2 is the output (heat) power. Constructing an equivalent equation with quasi-energy terms, equation (5) is obtained:

$$F = F_1 + F_2 \quad (5)$$

and therefore, $F_i = Q_i^2 w^{-1}$, where $i = 1, 2$.

From (4) and (5), $(Q_1 + Q_2)^2 = Q_1^2 + Q_2^2$

$$\text{or } Q_1 Q_2 = 0$$

In this case, either $Q_1 = 0$ or $Q_2 = 0$. If $Q = 0$, the biosystem is in a steady state; $Q = 0$ is the impossible state of no heat production.

Second, quasi-energy is additive only when Q is proportional to the body weight:

$$Q(w) = aw$$

where a is a constant.

Physical energy, on the other hand, is always additive:

$$E = (m_1 + m_2) = E(m_1) + E(m_2)$$

If the quasi-energies, Q_i , of two animal bodies weighing w_1 and w_2 are considered additive,

$$\frac{(Q_1 + Q_2)^2}{w_1 + w_2} = \frac{Q_1^2}{w_1} + \frac{Q_2^2}{w_2}$$

This equation is only satisfied when Q is a linear function of w , that is, when

$$Q(w) = aw \text{ or } F(w) = a^2 w$$

Thus the new bioenergetic quantity, $F = Q^2 w^{-1}$, resembles physical energy by virtue of both its dimensions and the structure of its definitive terms. It may be considered a new auxiliary concept in theoretical bioenergetics.

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¹ Mezghzerin, V. A., *Bionika. Modelirovanie Biosystem*, 139 (in Russian, Kiev, 1967).

² Dyul'din, A. A., and Mezghzerin, V. A., *Bionikai Matematicheskoe Modelirovanie Biosystem*, 109 (in Russian, Kiev, 1969).

³ Mezghzerin, V. A., and Dyul'din, A. A., *Nature*, **227**, 305 (1970).

Two Inducers of Rapid Erythroblast Multiplication *in vitro*

WHEN rabbit blood buffy coat and marrow cells were incubated together for 1 h at 37° C there was an increase in erythroblasts, greater in the basophils than in the acidophils. There was no change in marrow incubated alone, and buffy coat alone produced no erythroblasts. Autoradiographic experiments showed that the newly formed erythroblasts all came from marrow cells.

Granick and Kappas¹ discovered a group of derivatives of sex hormones which induced haem synthesis *in vitro* in chick embryonic tissue²⁻⁵, and they have given us four of their active compounds to test: Pregnanediol, 5 β Androstan 3 α -ol-17 one, 11-ketopregnanolone and 5 β -pregnan-3 α -17- α diol-11,20 dione. 11-Ketopregnanolone was active in our system, although not as active as with buffy coat. The other steroids were much less active.

Counting chamber or Coulter counter methods were not feasible in these experiments; basophils could not be distinguished from acidophils, erythroblasts from nucleated non-erythroid cells nor reticulocytes from erythrocytes. The increases in erythroblasts would have been obscured by the much larger number of nucleated non-erythroid cells. The cell count could therefore not be expressed in absolute numbers (for example, per ml.). To count the erythroblasts, at the end of the incubation a measured suspension of chicken erythrocytes as markers was added and the numbers of each kind of cell were compared with the number of chicken erythrocytes in the area of the smear counted.

If the induced increases of erythroblasts had been due to lysis of chicken erythrocytes by the leucocytes or steroid, there would have been the same apparent increases of non-nucleated erythroid cells, that is, they would seem to have been doubled or more. No increase, however, was observed. The observed and calculated counts agreed on the basis of no lysis of chicken erythrocytes. Furthermore, if the increased counts had been due to lysis of the chicken erythrocytes the increases in basophils and acidophils would have been in identical proportions; but the relative increases in basophils with leucocytes or steroid were often double or more those of the acidophils. Ghosts of chicken erythrocytes were seen rarely, were readily recognized and were included as such in the count.

The leucocyte preparation was a modification of that of Skoog and Beck⁶. Heparinized blood was mixed with an equal volume of 6% bovine fibrinogen (fraction 1, 65% clottable, from Pentex, Kankakee, Illinois 60901) in isotonic saline, and allowed to stand at room temperature for 50–60 min. The erythrocytes coalesced in rouleaux and settled rapidly. The supernatant was centrifuged at 900g for 10 min; the pellet from 150–300 ml. of blood was resuspended in 10 ml. of the supernatant in a 12 ml. conical centrifuge tube and again centrifuged. In this pellet, the erythrocytes were sharply layered below the leucocytes and removed with a Pasteur pipette. The leucocytes were suspended in 10 volumes of plasma and set aside at 10° C. In about 1 h the leucocytes had settled, leaving most of the erythrocytes which had not formed rouleaux suspended above. These were removed. In this, the final state of the preparation, the leucocyte/erythrocyte ratio was 3 : 10.

The incubation medium was rabbit plasma from blood to which had been added 50 U/ml. of heparin sodium (100 μ /mg); the plasma was heated at 56° C for 0.5 h and immediately before use penicillin G and streptomycin SO₄ (0.2 mg/ml. each) and oxytetracycline (0.05 mg/ml.) were added. In later experiments the plasma also contained the following metabolite mixture: glucose 1 mg/ml.; L-amino-acids, each at 5×10^{-4} M; the eight common deoxynucleosides, each at 5×10^{-5} M; insulin, 0.8 μ /ml.; FeSO₄·7H₂O, 0.55 mg/100 ml.; ascorbic acid, 2 mg/100 ml. and L-glutamine, 7 mg/100 ml. (The L-amino-acids were alanine, arginine, asparagine, cys-

teine, glycine, histidine, hydroxyproline, isoleucine, leucine, lysine, methionine, phenylalanine, proline, serine, threonine, tryptophan, tyrosine and valine.)

The metabolite mixture is not necessary (Tables 1 and 2). The erythroblast increases were a little larger when the metabolite was used, but they were too variable to allow definite conclusions to be drawn. Rabbit erythrocytes were added to all the reaction mixtures, making the cellular population resemble that of normal marrow, and facilitating equilibration of the oxygen tension with the overlying air. The erythrocyte content of the leucocyte preparations varied, and additions of a much larger quantity of erythrocytes reduced the differences in erythrocyte content in different reaction mixtures. Finally, the added erythrocytes facilitated the preparation of more uniform smears.

The cell mixture suspended in 1 ml. consisted (as packed cell volume) of washed rabbit erythrocytes, 0.025 ml., marrow cells, 0.01 ml. (about 10^7 cells), and leucocytes (when used), 0.1 ml. (about 10^8 cells). Steroid additions were made in ethanol, 10 μ l.

The incubation procedure has been described before⁷; at the end of the hour at 37° C the beakers are left with 5 U of erythropoietin at 10° C overnight. This step is not essential; the principal effects had occurred within 1 h at 37° C without erythropoietin. (Erythropoietin was procured by the Department of Physiology, University of the Northeast, Corrientes, Argentina, and processed by the Hematology Research Laboratories, Children's Hospital of Los Angeles, for distribution by the National Heart Institute. It was authorized for distribution by the Committee on Erythropoietin of the National Heart Institute.) It was done partly for convenience—the erythroblasts remained in good condition—and partly

because we had had indications of some maturation overnight at 10° C with erythropoietin. At the end of the incubation, whether after 1 h at 37° C or overnight at 10° C, 0.5 ml. of a washed chicken erythrocyte suspension diluted 200-fold in isotonic saline was added. The smears were stained with benzidine-Giemsa⁸.

Tables 1 and 2 give results obtained with a marrow fraction in which responsive cells are concentrated. It was obtained by centrifugation of the marrow in a bovine serum albumin (BSA) density gradient⁸. Results with whole marrow are in Tables 3 and 4. Counts were made at zero time and at intervals in several experiments. Without leucocytes or steroid, there were no changes from the zero time numbers of erythroblasts up to 1 h at 37° C followed by overnight at 10° C. Accordingly, the comparisons in the tables are with and without additive. The whole marrow was prepared in the same way as for the fractionation on the BSA gradient.

Table 3 Increase in Erythroblasts of Whole Marrow Effects of Leucocytes and 11-Ketopregnanolone (Steroid)

Exp.	Basophils: additives				Acidophils: additives			
	None	Leuco- cytes	Steroid	Leuco- cytes and steroid	None	Leuco- cytes	Steroid	Leuco- cytes and steroid
1	1.3	2.8			13.8	18.5		
2	0.8	5.0			4.5	5.3		
3	2.5	5.2	4.9	4.8	12.1	12.6	12.5	12.6
4	1.4	13.7	4.4	15.6	13.1	21.9	18.5	23.5

Values are erythroblasts per 100 chicken erythrocytes. Metabolite mixture was used. Steroid was in a concentration of 6 μ g/ml.

Table 1 Increase in Erythroblasts in Fraction 1+2 of Marrow Cells fractionated on BSA Gradient⁸

No. of comparisons	Basophils R_B		Acidophils R_A		$\frac{R_B}{R_A}$	
	Range of values	%	Range of values	%	Range of values	%
70	1.0-1.9	20	1.0-1.5	40	0.4-0.9	4
	2.0-2.9	46	1.6-2.0	37	1.0-1.4	44
	3.0-3.9	18	2.1-2.5	12	1.5-1.9	28
	4.0-5.9	4	2.6-3.0	7	2.0-2.4	13
	6.0-19.6	11	3.0-3.7	4	2.4-7.7	10
Mean $\pm$ s.d.	3.5 $\pm$ 1.85		1.9 $\pm$ 1.0		1.6 $\pm$ 0.7	
Range of initial concentration: per 100 chicken erythrocytes	2.3-61.0		0.3-68.8			

No metabolite mixture was used.

$R_B = \frac{\text{Basophils after incubation with leucocytes}}{\text{Basophils after incubation without leucocytes}}$
 $R_A = \frac{\text{Acidophils after incubation with leucocytes}}{\text{Acidophils after incubation without leucocytes}}$

Table 2 Increase in Erythroblasts in Fractions 1+2 of Marrow Cells fractionated on BSA Gradient⁸

Exp.	Basophils: additive				Acidophils: additive			
	None	Leuco- cytes	Ster- oid	Leuco- cytes and steroid	None	Leuco- cytes	Ster- oid	Leuco- cytes and steroid
1	46.0	80.4	82.3	100.5	15.1	18.7	20.0	18.1
2	8.0	51.1	42.6	48.9	11.2	18.0	21.2	21.3
3	21.1	34.9	30.4	34.5	15.4	14.8	15.9	14.5
4	21.4	36.2	29.9	40.8	36.5	39.4	35.3	47.5

This table shows a comparison of the effects of leucocytes and 11-ketopregnanolone (steroid). Values are erythroblasts per 100 chicken erythrocytes.

Metabolite mixture was used. Steroid was in a concentration of 6 μ g/ml.

Table 4 Variation in Relative Erythroblast Increase dependent mainly on the Marrow Cells

Source of marrow	Basophils Leucocytes from			Acidophils Leucocytes from		
	None	A+B	C+D	None	A+B	C+D
A	1.9	9.3	7.3	17.8	35.9	30.0
B	0.7	4.0	4.1	4.9	7.3	7.4
C	1.3	3.5	2.8	13.8	18.1	18.5
D	0.8	2.5	3.0	4.5	4.5	5.3

Values are whole marrow erythroblasts per 100 chicken erythrocytes. Metabolite mixture was used.

In all four sets of experiments (Tables 1-4), leucocytes or steroid increased the erythroblasts, more for basophils than for acidophils. The increases with leucocytes tended to be larger than with steroid, and were not additive when the two were together. Variability of response in marrow fractions 1+2 was similar to that encountered in whole marrow. Variability was due chiefly to variation from animal to animal in the marrow cells; it was similar with both leucocytes and steroid (Tables 2 and 4). Similar results were also observed with autologous and homologous mixtures of fraction 1+2 and leucocytes prepared from individual animals.

We have not positively identified the active cells among the leucocytes, although autoradiographic experiments point to the large lymphocytes. These experiments, done only with leucocytes as stimulator, also indicated that some cell division of pre-existing basophils and acidophils probably occurred, but apparently not enough to account for the observed increases, especially in the basophils, which in most cases more than doubled (Table 1). There were enough nucleated, non-erythroid cells at zero time (and later in the preparations without added leucocytes or steroid) to account for the observed increases in erythroblasts. It could not, however, be determined which, if any, of these were the precursors. The actual precursors of the erythroblasts increased by leucocytes could

not be determined in our experiments because too many leucocyte cells were like marrow non-erythroid cells. The stimulating effect of the steroid made the investigation of the precursors of the increased erythroblasts feasible, and it is now in progress. The preliminary indications are that generation as well as division of non-erythroid precursors occur.

Erythropoiesis in the adult animal has been likened in some respects to embryonic development⁹. There are two more points of analogy in the observations described above: (i) the leucocyte-induced increase in erythroblasts and the potentiation of embryonic differentiation in one type of cell by the action on it of another; and (ii) the inducing action of 11-keto-pregnanolone on rabbit marrow cells and the chick embryo¹⁻⁵.

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- ¹ Granick, S., and Kappas, A., *J. Biol. Chem.*, **242**, 4587 (1967).
- ² Granick, S., *J. Biol. Chem.*, **241**, 1359 (1966).
- ³ Granick, S., and Kappas, A., *Proc. US Nat. Acad. Sci.*, **57**, 1463 (1967).
- ⁴ Levene, R. D., Kappas, A., and Granick, S., *Proc. US Nat. Acad. Sci.*, **58**, 985 (1967).
- ⁵ Kappas, A., and Granick, S., *J. Biol. Chem.*, **243**, 346 (1968).
- ⁶ Skoog, W. A., and Beck, W. S., *Blood*, **11**, 436 (1956).
- ⁷ Borsook, H., Ratner, K., Tattrie, B., Tiegler, D., and Lajtha, L. G., *Nature*, **217**, 1024 (1968).
- ⁸ Borsook, H., Ratner, K., and Tattrie, B., *Blood*, **34**, 32 (1969).
- ⁹ Borsook, H., *Biol. Rev.*, **41**, 259 (1966).
- ¹⁰ Holftreter, J., *Growth*, **15**, Suppl., 117 (1951).
- ¹¹ Holftreter, J., and Hamburger, V., in *Analysis of Development* (edit. by Willier, B. H., Weiss, P. A., and Hamburger, V. I.), Section 7, ch. 1 (1955).
- ¹² Grobstein, C., *J. Morphol.*, **93**, 19 (1953).
- ¹³ Grobstein, C., *J. Exp. Zool.*, **124**, 383 (1953).
- ¹⁴ Grobstein, C., *Science*, **118**, 52 (1959).
- ¹⁵ Grobstein, C., *J. Exp. Zool.*, **130**, 319 (1955).
- ¹⁶ Grobstein, C., *Exp. Cell Res.*, **10**, 424 (1956).
- ¹⁷ Grobstein, C., *Nature*, **172**, 869 (1953).
- ¹⁸ Grobstein, C., and Dalton, A. J., *J. Exp. Zool.*, **135**, 57 (1957).
- ¹⁹ Saxen, L., *Develop. Biol.*, **3**, 140 (1961).
- ²⁰ Sundelin, P., Wartiovaara, J., Saxen, L., and Thorell, B., *Exp. Cell Res.*, **54**, 347 (1969).
- ²¹ Miura, Y., and Wilt, F. H., *Develop. Biol.*, **19**, 201 (1969).

Catecholamine Metabolism, Depression and Stress

In recent years, the catecholamine hypothesis for affective or emotional disorders has gained considerable currency¹⁻³. According to this hypothesis some, if not all, states of mental depression may be associated with a deficiency in norepinephrine (NE) at functionally important receptor sites in the brain. We have assayed the products of catecholamine metabolism that appear in the urine of patients in the hope of obtaining information about catecholamine disposition in the affective disorders.

A major metabolite of brain NE is 3-methoxy-4-hydroxy-phenylglycol (MHPG) and, in dogs, 25-30% of the urinary MHPG originates in the brain pools of NE. In contrast, most of the urinary 3-methoxy-4-hydroxymandelic acid (VMA) and the normetanephrine (NM) are products of catecholamine breakdown in peripheral body pools⁴⁻⁷. In a pilot study we found that a number of depressed patients had concentrations of MHPG in their urine which were

significantly smaller than those in the urine of normal people⁸. The concentrations of NM and metanephrine, on the other hand, were the same. Since then others have also found that MHPG levels seem to be associated with decreased depression, whereas in the manic state the amount of MHPG excreted is normal^{9,10}. There are several plausible ways, however, by which a decrease in urinary MHPG can be explained without invoking a change in the disposition of NE in the central nervous system¹¹. For example, it could be argued that depressed patients have smaller concentrations of MHPG in their urine because of deficiencies in alcohol reductase, NADH₂, or substrate accessibility, all of which would decrease the amount of MHPG formed in unit time. Alternatively, noradrenergic and/or adrenergic systems outside the central nervous system may be functionally less active in the depressed patients than in normal subjects. Here we provide evidence against these alternative explanations and for a relationship between stress and MHPG formation.

Six severely depressed patients were selected. Five had psychotic depressions and one was of the manic-depressive reaction, depressed type. The ages of the patients ranged from 49 to 62 years and five of the six were males. After spending 2 weeks in a metabolic research unit the patients were infused continuously for a period of 48 h with ³H-NE by a technique which has been described in detail elsewhere¹² but which may be briefly outlined as follows. A small portable pump was strapped to the subject's arm and ³H-NE in tracer quantities was continuously infused (0.7 ml. solution h⁻¹) for 48 h through a catheter implanted in the antecubital vein. Urine was collected at intervals during the infusion and for 48 h after its cessation. In addition, 24 h urine specimens were collected on the seventh or eighth day in hospital, that is, approximately one week before the infusion study. (Data on the excretion of ³H-NE and ³H-metabolites are being analysed and prepared for publication.) A control group of five physically healthy young male subjects were housed in another metabolic unit and, like the patients, these subjects were fed a diet free of coffee, tea, colas, vanilla, bananas, cheese, oranges and chocolate. Again, 24 h urine specimens were collected for assay on the seventh or eighth day after admission to the unit. Neither group received medication for at least three weeks before participating in these studies. Endogenous (not isotopically labelled) catecholamine metabolites were assayed by the following methods: NM and M by the method of Taniguchi *et al.*¹³, VMA by a modification of the gas-liquid chromatographic method of Wilk *et al.*¹⁴, and MHPG by a gas-liquid chromatographic method¹⁵ based on that described by Wilk *et al.*¹⁶.

The urinary concentrations of the endogenous metabolites before, during and after the infusion are shown in Fig. 1. Note that before the infusion study the urinary concentrations of MHPG in the depressed group of patients are significantly smaller than those found in the group of control subjects ($P=0.01$, one-tailed test) whereas the values for M, NM, and VMA are similar to those found in normal subjects. These data are in agreement with those reported in the earlier pilot study⁸ and extend the original findings in that measures of urinary VMA have been found to be similar for the two groups. The similarities in the M, NM and VMA urinary concentrations in patients and controls argue against the possibility that the decreased urinary MHPG concentration in the depressed group is caused solely by a decreased activity of adrenergic and/or noradrenergic systems in peripheral body pools. On summation of the VMA, NM and M concentrations, it was found that the control subjects excreted $2,222 \pm 371$ $\mu\text{g}/24$ h while the depressed patients excreted $2,097 \pm 313$ $\mu\text{g}/24$ h and that the differences between the means for the two groups are not statistically significant.

On the second day of the infusion study (see Fig. 1) the concentrations of all the metabolites had increased and it seems reasonable to conclude that the infusion causes a state of stress in the patients. Because tracer quantities of ³H-NE were

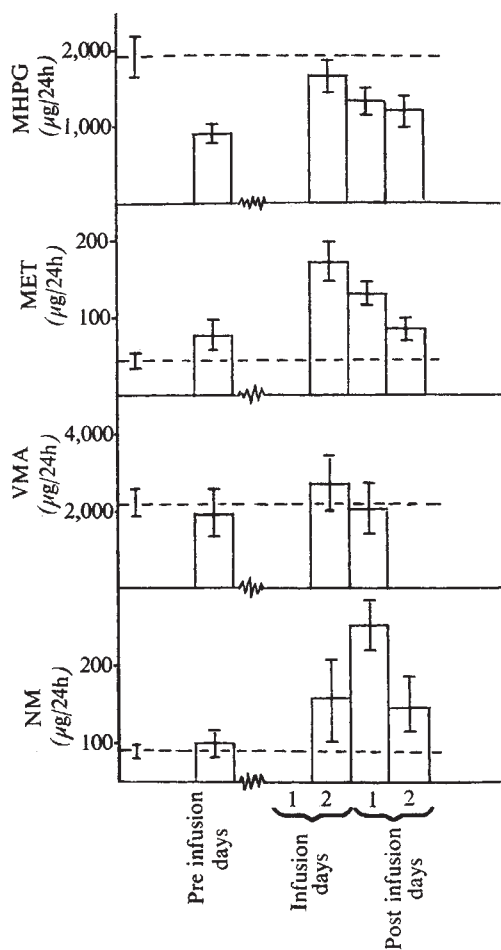


Fig. 1 Catecholamine metabolites excreted in urine by depressed patients before, during and after stress caused by continuous intravenous infusion over a 48 h period (see text). Mean values for the patient group ($n=6$) are shown as bars and expressed as μg excreted /24 h $\pm$ s.e.m. The mean values for a group of five subjects who were without symptoms of psychiatric illness are drawn as the horizontal dashed line (---) with the vertical bar representing the s.e.

infused (on average 1.634 $\mu\text{g}/48$ h) the increases in metabolites cannot be due to the amount of exogenous amine administered. Consonant with this interpretation is the tendency of the metabolite concentrations to return to baseline values during the first and second days after infusion. During the infusion period the MHPG values in the patient group are similar to those of the control subjects. As noted earlier, the smaller urinary MHPG concentrations in depressed patients the day before infusion could be interpreted in terms of enzyme and cofactor availability and a change in the capacity to form MHPG in catecholamine pools outside brain. While our data do not completely disprove this explanation, it is clear that depressed patients have the potential to form normal amounts of MHPG.

The statistical significance of the differences in the quantities of metabolites excreted during the pre-infusion and second infusion days was checked by the use of a paired "t" test; for MHPG, $P \approx 0.02$; M, $P \approx 0.05$; NM, $P \approx 0.10$ and VMA, $P > 0.50$. Thus it seems that MHPG formation is particularly susceptible to stresses in which adrenergic and/or noradrenergic systems—whether central, peripheral or both—are activated. In Fig. 1, for example, it can be seen that MHPG and M concentrations change most between the preinfusion and infusion days. In agreement with this is the recent report by Rubin *et al.*, who found significant elevations in urinary MHPG levels in naval aviators who were stressed by flight procedures¹⁷. Because MHPG is a minor metabolite of epinephrine in man¹⁸ the biological mechanisms responsible

for the marked increase in urinary MHPG are not clear and await elucidation by future studies. In contrast to MHPG the changes in VMA were small and variable. This finding is consistent with the hypothesis that much of intraneuronal NE is broken down to VMA (ref. 19) and that the level of this metabolite may be a relatively poor indicator of functional activity in catecholaminergic systems. Finally, these data, in conjunction with those of Sandler and Youdim, who found differing patterns of excretion of VMA and MHPG in subjects who had been given reserpine²⁰, suggest that the formation of MHPG and VMA involves more than a simple change in the relative rates of oxidation and reduction of the postulated aldehyde precursor.

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- Bunney, jun., W. E., and Davis, J. M., *Arch. Gen. Psychiat.*, **13**, 483 (1965).
- Schildkraut, J. J., *Amer. J. Psychiat.*, **122**, 509 (1965).
- Schildkraut, J. J., and Kety, S. S., *Science*, **156**, 21 (1970).
- Mannarino, E., Kirshner, N., and Nashold, B. S., *J. Neurochem.*, **10**, 373 (1963).
- Schanberg, S. M., Schildkraut, J. J., and Kopin, I. J., *Biochem. Pharmacol.*, **17**, 247 (1968).
- Maas, J. W., and Landis, D. H., *Psychosomatic Med.*, **28**, 247 (1966).
- Maas, J. W., and Landis, D. H., *J. Pharmacol. Exp. Therap.*, **163**, 147 (1968).
- Maas, J. W., Fawcett, J., and Dekirmenjian, H., *Arch. Gen. Psychiat.*, **19**, 129 (1968).
- Schildkraut, J. J., in *NIMH Workshop on the Biology of Depression*, 7 (US Government Printing Office, in the press).
- Goodwin, F. K., Brodie, H. K., Murphy, D. L., and Bunney, W. E., presented at the Annual Meeting, Soc. of Biol. Psychiat., San Francisco, May 1970.
- Maas, J. W., Fawcett, J., and Dekirmenjian, H., in *NIMH Workshop on the Biology of Depression*, 7 (US Government Printing Office, in the press).
- Maas, J. W., Benensohn, H., and Landis, D. H., *J. Pharmacol. Exp. Therap.*, **174**, 381 (1970).
- Taniguchi, K., Kakimoto, Y., and Armstrong, M. D., *J. Lab. Clin. Med.*, **64**, 469 (1964).
- Wilk, S., Gitlow, S. E., Mendlowitz, M., Franklin, M. J., Carr, H. E., and Clarke, D. D., *Anal. Biochem.*, **13**, 544 (1965).
- Dekirmenjian, H., and Maas, J. W., *Anal. Biochem.*, **35**, 113 (1970).
- Wilk, S., Gitlow, S. E., Clarke, D. D., and Paley, D. H., *Clin. Chim. Acta*, **16**, 403 (1967).
- Rubin, R. T., Miller, R. G., Clark, B. R., Poland, R. E., and Arthur, R. J., *Psychosomatic Med.*, **32**, 589 (1970).
- LaBrosse, E. H., Axelrod, J., Kopin, I. J., and Kety, S. S., *J. Clin. Invest.*, **40**, 253 (1961).
- Kopin, I. J., and Gordon, E., *J. Pharmacol. Exp. Therap.*, **140**, 207 (1963).
- Sandler, M., and Youdim, M. B. H., *Nature*, **217**, 772 (1968).

Effects of L-Dopa on Efflux of Cerebral Monoamines from Synaptosomes

THE mechanism of the effects of large doses of L-dopa on brain function is ill-defined. This form of precursor loading produces marked increases in brain dopamine, little or no change in noradrenaline, and a substantial decrease in serotonin with an initial increase in 5-H indolyl-3-acetic acid (5-HIAA)¹⁻³. These observations suggest that serotonin may be released by L-dopa or its metabolites. Isolated synaptosomes (pinched-off nerve

endings) actively accumulate exogenously labelled amines into structures which normally contain these amines⁴⁻⁶. Although the distribution of the labelled amine in the endogenous stores is not clearly defined, these structures seem to provide valid models for nerve endings in brain and may afford a direct and sensitive means of assessing the influence of drugs on the release of neurohumours. Using this technique, we have found that L-dopa profoundly affects the efflux of cerebral monoamines.

Synaptosomes were prepared from whole rat brain by the method of Whittaker *et al.*⁷, except that only broken cells and nuclei were separated from the synaptosomes. This "crude" preparation was sufficient, for myelin, mitochondria and microsomes take up only a small fraction of the monoamines taken up by the particulate fraction, most accumulating in the synaptosomes. Crude synaptosomes were transferred to incubation medium (5 ml./50 mg of whole brain used) containing 118 mM NaCl, 4.7 mM KCl, 2.2 mM CaCl₂, 1.18 mM MgSO₄, 11 mM dextrose, and 25 mM Na₃PO₄, pH 7.0. The synaptosome preparation was then incubated with ¹⁴C-noradrenaline, 4×10^{-7} M (40 mCi/mmol), ³H-dopamine, 2×10^{-8} M (10 Ci/mmol), or ¹⁴C-serotonin, 1×10^{-7} M (56 mCi/mmol) for 20 min at 37° C, and uptake was stopped by cooling the tubes on ice. After centrifugation at 16,000g for 10 min at 4° C, the supernatant was discarded and the synaptosomal pellets were resuspended in 3 ml. of fresh incubation medium and incubated at 37° C for 5 min to allow equilibration. MK-485 (1×10^{-4} M) was added to some synaptosomal preparations during equilibration, after which 0.1 ml. of incubation medium containing L-dopa (3×10^{-4} M) was added so that the final concentration was 10^{-5} M. After incubation for a further 20 min at 37° C, the samples were cooled on ice and centrifuged at 16,000g for 10 min. The supernatant was removed and the pellets were homogenized with 250 μ l. of 0.4 N perchloric acid, centrifuged and 200 μ l. of the supernatant was transferred to vials for assay of total radioactivity by liquid scintillation spectrometry.

In other experiments, the formation of ³H-dopamine from ³H-L-dopa and the effect of decarboxylase inhibition (MK-485) was determined. Synaptosomes were prepared as described. MK-485 (10^{-4} M) was added to some samples during the 5 min equilibration period. After equilibration, 0.1 ml. of incubation media containing ³H-L-dopa (2.5 μ Ci) was added with cold L-dopa to give a final concentration of 10^{-5} M (5 μ Ci/mmol). After incubation for 20 min at 37° C, the reaction was stopped by cooling the tubes on ice. Synaptosome pellets were recovered and extracted with 0.4 N perchloric acid as before. Dopa and dopamine in these pellets were determined by adsorption on alumina followed by ion-exchange on 'Dowex-50' (Na⁺ form) and differential elution as described earlier^{8,9}.

L-dopa produced a significant increase in efflux of labelled monoamines from synaptosomes previously incubated with ³H-dopamine, ¹⁴C-noradrenaline, or ¹⁴C-serotonin (Table 1). Analysis by column chromatography ('Dowex-H⁺')⁸ showed that the labelled efflux induced by L-dopa consisted predom-

Table 1 Effect of L-Dopa or L-Dopa+MK-485 on Efflux of Monoamines from Synaptosomes

	Total radioactivity		
	¹⁴ C-Noradrenaline	³ H-Dopamine	¹⁴ C-Serotonin
Control	2,331 $\pm$ 35 *	14,322 $\pm$ 356	1,064 $\pm$ 14
Control+L-Dopa (1×10^{-5} M)	1,946 $\pm$ 9 †	8,096 $\pm$ 218 †	706 $\pm$ 52 †
Control+L-Dopa (1×10^{-5} M)+MK-485 (1×10^{-4} M)	2,287 $\pm$ 18	13,058 $\pm$ 101	920 $\pm$ 12

* Results represent levels of radioactivity ($\pm$ s.e.) remaining in synaptosome pellet expressed in c.p.m./synaptosome from 50 mg of whole brain for groups of four synaptosome preparations.

† $P < 0.001$ compared with control.

Table 2 Effect of Decarboxylase Inhibition (MK-485) on Dopamine Formation from ³H-L-Dopa by Synaptosomes

	³ H-L-Dopa	³ H-Dopamine
Control	14,202 $\pm$ 1,105 *	742 $\pm$ 85
MK-485	16,096 $\pm$ 302	207 $\pm$ 25 †

* Results are levels of tritium ($\pm$ s.e.) present in the indicated compound expressed as c.p.m./synaptosome from 50 mg of whole brain for groups of four synaptosomal preparations.

† $P < 0.001$ compared with control.

inantly of the unmetabolized amine. The addition of an aromatic L-amino-acid decarboxylase inhibitor, alpha-methyl-dopa hydrazine (MK-485), to the incubation medium, at a concentration which markedly reduced the formation of ³H-dopamine from ³H-L-dopa (Table 2), significantly reduced the releasing action of L-dopa (Table 1). MK-485 alone at the concentration used did not influence significantly the uptake of L-dopa or alter the spontaneous efflux of the labelled monoamines. L-Dopa is largely decarboxylated to dopamine in brain, and so it seems likely that the efflux of monoamines induced by L-dopa is mediated by conversion to dopamine.

Previous studies have shown that exogenous noradrenaline¹⁰, dopamine¹¹ and serotonin¹² are accumulated *in vitro* by brain slices which have specific uptake systems¹³. These exogenous amines localize in synaptosome fractions of homogenates^{4-6,14,15}, which suggests that exogenous amines label the endogenous pools, although it is not clear if the exogenous label is uniformly distributed among the intrasynaptosomal pools¹⁶. It may well be that the behaviour of exogenous label differs from that of the endogenous amine. Nevertheless, the effects of drugs on the efflux of cerebral monoamines from isolated synaptosomes are probably a useful model of their physiological (or pharmacological) action at central synapses. According to this model, the results show that L-dopa enhances the efflux of cerebral monoamines and that this action is contingent on the decarboxylation of L-dopa to dopamine. These results are consistent with our observation that L-dopa releases cerebral monoamines from brain slices¹⁷.

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- Butcher, L. L., and Engel, J., *Brain Res.*, **15**, 223 (1969).
- Bartholini, G., DaPrada, M., and Pletscher, A., *J. Pharm. Pharmacol.*, **20**, 228 (1968).
- Everett, G. M., and Borcherding, J. W., *Science*, **168**, 849 (1970).
- Colburn, R. W., Goodwin, F. K., Murphy, D. L., Bunney, jun., W. E., and Davis, J. M., *Biochem. Pharmacol.*, **17**, 957 (1968).
- Aghajanian, G. K., and Bloom, F. E., *J. Pharmacol.*, **156**, 23 (1967).
- Coyle, J. T., and Snyder, S. H., *J. Pharmacol.*, **170**, 221 (1969).
- Whittaker, V. P., Michaelson, I. A., and Kirkland, R. J. A., *Biochem. J.*, **90**, 293 (1964).
- Häggendal, J., *Scand. J. Clin. Lab. Invest.*, **14**, 537 (1962).
- Anton, A. H., and Sayre, D. F., *J. Pharmacol.*, **138**, 360 (1962).
- Dengler, H. J., Michaelson, I. A., Spiegel, H. E., and Titus, E., *Intern. J. Neuropharm.*, **1**, 23 (1962).
- Baldessarini, R. J., and Kopin, I. J., *Science*, **152**, 1630 (1966).
- Blackburn, K. J., French, P. C., and Merrills, R. J., *Life Sci.*, **6**, 1653 (1967).
- Shaskan, E. G., and Snyder, S. H., *J. Pharmacol.*, **175**, 404 (1970).
- Iversen, L. L., and Snyder, S. H., *Nature*, **220**, 796 (1968).
- Snyder, S. H., Kuhar, M. J., Green, A. I., Coyle, J. T., and Shaskan, E. G., *Intern. Rev. Neurobiol.*, **13**, 127 (1970).
- Glowinski, J., and Iversen, L. L., *Biochem. Pharmacol.*, **15**, 977 (1966).
- Ng, K. Y., Chase, T. N., Colburn, R. W., and Kopin, I. J., *Science*, **170**, 76 (1970).

Patchy Retinal Degeneration in Tetraparental Mice

TETRAPARENTAL mice, formed by combining two eight-cell stage embryos into one, provide a unique opportunity for studying mammalian developmental phenomena^{1,2}. When the two embryos forming a given mouse originate from different inbred strains, the adult offspring displays chimaerism in many tissues including skin², liver³ and blood^{4,5}, if the two parental strains differ at a genetic locus expressed in the tissue under examination. The pattern of mosaicism can provide information on how the tissue developed. Although chimaerism in the nervous system would clearly be of great interest both from morphogenetic and physiological points of view, it has not until now been demonstrated in these mice, as far as we are aware.

In order to look for chimaerism in the central nervous system, we chose C57Bl/10SnJ-C3H/HeJ tetraparental mice, because the C3H/HeJ parental strain is homozygous for a recessive gene causing retinal degeneration (*rd*), whereas the C57Bl/10SnJ strain carries the wild type allele at this locus⁶. When the *rd* gene is present in homozygous form, retinal development proceeds indistinguishably from normal only until about the eleventh postnatal day. By that stage all eight to ten rows of photoreceptor nuclei have formed, and the outer segments have just begun to appear. Outer segment development then ceases and the rod nuclei, inner segments, and outer segments begin to degenerate. By 25–30 days of age only one sparsely populated row of photoreceptor nuclei remains and the outer segments have disappeared.

The eyes from each of three C57-C3H tetraparental mice chimaeric for coat colour and haemoglobin were removed and immersed in 2.5% glutaraldehyde in 0.1 M Sorensen's phosphate buffer, pH 7.3. At the same time eyes from non-chimaeric litter mates, which were purely of one parental type with reference to coat colour and haemoglobin, were removed and fixed as controls. Half of each eye was embedded in polyester wax and serially sectioned at a thickness of 8 microns. The other half was embedded in glycol methacrylate or in an 'Epon-Araldite' mixture, and sectioned semi-serially at 1–2 μ m.

Mouse L-288-1 was 26 days old when the eye was removed. Her coat was almost entirely of C3H colour (agouti), with a few spots of C57 colour (black) on her forehead. Her haemoglobin mix was 54%; 46% C3H; C57 (95% fiducial limits = $\pm 5\%$), based on the beta chain polymorphism measured by densitometry following starch gel electrophoresis. Both retinas had areas or "patches" of degeneration interspersed with areas of partially affected or normal tissue. In the areas of degenera-



Fig. 2 This field, from the retina of another tetraparental mouse, shows the normal eight to ten rows of photoreceptor nuclei at the right edge. The number of rows decreases to nought to two in the left half of the picture. The rod outer segments, which are darkly stained in this osmium-treated specimen, gradually decrease in length approximately in parallel with the reduction in nuclei. PN, Photoreceptor nuclei; OS, rod outer segments. Eye fixed in glutaraldehyde, postfixed in osmium tetroxide, embedded in an 'Epon-Araldite' mixture, sectioned at 1 μ m, and stained with toluidine blue. The bar represents 20 μ m.

tion only zero to two rows of photoreceptor nuclei were present, and the outer segments were absent (Fig. 1, arrow). The areas of degeneration bordered on intermediate areas, which contained between four and six rows of photoreceptor nuclei instead of the usual eight to ten rows, and had outer segments of intermediate length (Fig. 1, double arrow). These intermediate areas, which predominated in this mouse, graded occasionally into small normal patches by a steady increase both in the number of rows of rod nuclei and in the length of the outer segments.

Mouse L-288-6, enucleated when 30 days old, had approximately 80% C57 coat colour, and a haemoglobin which was 85–95% C57 in type (the estimate is more tentative with a mix containing C57 haemoglobin $\geq 85\%$). Most of the tissue in both retinas appeared completely normal, with eight to ten rows of photoreceptor nuclei and outer segments of normal length. A few diseased patches were seen, however, including a number involving a portion of the ora serrata of one eye (Fig. 2). The diseased patches looked identical to those in mouse L-288-1. To obtain a two dimensional representation of the patches over a large area of retinal surface, the outlines of 1 μ m sections at 10 μ m intervals were traced with a camera lucida. Degenerated, intermediate, and normal areas were plotted by direct observation with the microscope. Fig. 3 shows the results of one such mapping, carried out in the affected region of the ora serrata. Dotted lines indicate degenerated areas (nought to two rows of photoreceptor nuclei), dashed lines indicate intermediate areas (three to six rows), and solid lines indicate normal areas (eight to ten rows). The left border of the diagram represents the ora serrata. The lower portion of this region is degenerated, the middle is intermediate, and the upper half is normal. There is a normal patch in the lower middle section of the diagram which has an approximately circular shape and a diameter of about 25 μ m. It is circumscribed by an intermediate zone about 20 μ m wide. The isolated 8 μ m patch of degeneration seen in the upper right hand corner is typical of patches found elsewhere in this eye. Ten such isolated patches had diameters between 8–12 μ m. All degenerated patches examined were surrounded by intermediate zones.

Mouse L-288-2, 26 days old, had roughly equal amounts of C57 and C3H coat colour, but the haemoglobin pattern was overwhelmingly C57 (about 5% C3H). No degenerated or intermediate patches were seen in an extensive series of sections through the retinas of this animal. Retinas from 84 control animals had the morphology expected on the basis of their coat

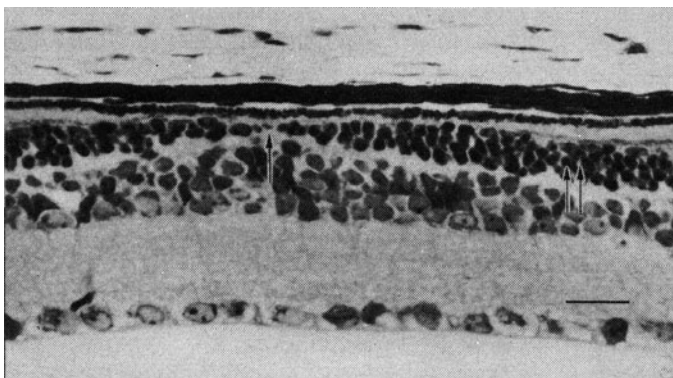


Fig. 1 This field of mouse retina shows a small patch (arrow) in which photoreceptor cells have degenerated so that only nought to two rows of photoreceptor nuclei remain. The number of rows gradually increases to four or five in the adjacent intermediate patches (double arrow). The eye was fixed in glutaraldehyde, embedded in glycol methacrylate, sectioned at 2 μ m and stained with toluidine blue. The bar represents 20 μ m.

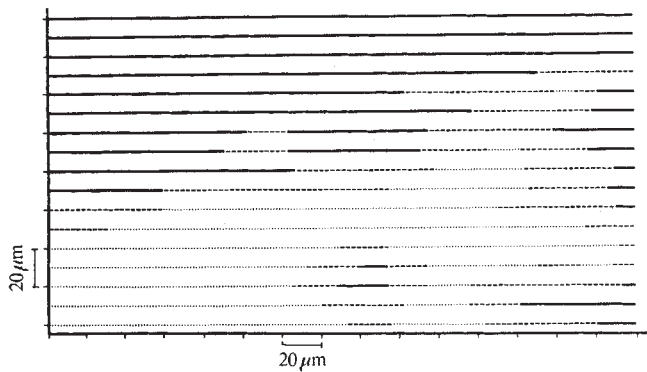


Fig. 3 Map of an area of retinal surface to show the two dimensional distribution of patches of degenerated (dotted lines), intermediate (dashed lines), and normal (solid lines) photoreceptor cells. A given section lies in the horizontal axis of the map; every tenth 1 μ m section was plotted. See text for further description.

colour and haemoglobin patterns. Many additional retinas from non-fused mice showed no chimaerism.

Tarkowski has reported that tetraparental mice composed of two strains, one of which lacks ocular pigment, have a somewhat patchy pattern of pigment epithelium in the eye⁷. The pigment epithelium is a derivative of the embryonic optic vesicle. The results reported here indicate that in tetraparental mice the neural tissue of the eye also can display a patchy pattern (B. Mintz also has seen retinal patches in comparable mice—personal communication, 1967). An unexpected finding was the intermediate zone between the degenerated and normal areas. An intermediate phenotype in a different organ system, that is, tail length, has been described too⁸. Experiments are being conducted to analyse patch morphology in further detail and to examine the relation between the pigment epithelial and retinal patches, using mice of the appropriate genotypes. The current results warrant the prediction that tetraparental mice will prove most valuable in studies of neural development and function.

Since this communication was submitted Mintz's observations referred to here have appeared in abstract form⁹.

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¹ Tarkowski, A. K., *Nature*, **190**, 857 (1961).

² Mintz, B., *Proc. US Nat. Acad. Sci.*, **58**, 344 (1967).

³ Wegmann, T. G., *Nature*, **225**, 462 (1970).

⁴ Wegmann, T. G., and Gilman, J. G., *Develop. Biol.*, **21**, 281 (1970).

⁵ Mintz, B., and Palm, J., *J. Exp. Med.*, **129**, 1013 (1969).

⁶ Sidman, R. L., and Green, M. C., *J. Hered.*, **56**, 23 (1965).

⁷ Tarkowski, A. K., *J. Nat. Cancer Inst. Monog.*, No. 11, 51 (1963).

⁸ McLaren, A., and Bowman, P., *Nature*, **224**, 238 (1969).

⁹ Mintz, B., and Sangal, S., *Genetics*, **64**, Suppl. 43 (1970).

Parafollicular Cells in the Normal Human Thyroid

NONIDEZ¹ has described argyrophillic, granulated cells distinct from follicular cells in the dog thyroid. It is now known that the mammalian thyroid is a source of calcitonin², and the parafollicular cells seem to produce thyrocalcitonin^{3,4}.

Although calcitonin activity has been demonstrated in human thyroid tissue⁵, the parafollicular cells of the human thyroid have never before been demonstrated. Their presence has been assumed, however, because medullary carcinoma of the thyroid consists of cells resembling the parafollicular cells of other mammalian species, and contains calcitonin activity⁶.

A thyroid lobectomy was performed on a 26 yr old female for a solitary adenomatous nodule. Random pieces of grossly normal thyroid tissue from the specimen were diced into 0.5 to 1.0 mm blocks, fixed in 1.5% glutaraldehyde in phosphate buffer (pH 7.4), post-osmicated, dehydrated and embedded in 'Araldite 502' (Ciba). Thick sections were stained with 1% toluidine blue to localize parafollicular cells. Thin sections were stained with lead citrate and uranyl acetate.

Areas suggesting the presence of parafollicular cells were found when more than 200 random sections had been examined histologically (Fig. 1). By light microscopy the cells were identical to those of other mammalian thyroids. They were approximately 1.5–2 times as large as follicular cells and were clustered within the follicular basement membrane, but did not abut on the colloid. The nuclei appeared somewhat more vesicular than those in follicular cells. Large numbers of darkly staining granules were present within the cytoplasm of the cells.

When an area suspected of containing parafollicular cells had been located histologically, the next section was cut from the block for electron microscopy. Ultrastructurally, as well as by light microscopy, the human parafollicular cells were similar to those of other mammals (Figs. 2 and 3). They were easily distinguished from follicular cells by an abundance of membrane bound, electron dense, secretory granules, 200–300 Å in diameter. Furthermore, whereas the follicular cell is characterized by large quantities of dilated, rough, endoplasmic reticulum, the parafollicular cell contained abundant ergastoplasm, lined by ribosomes and stacked on itself in a characteristic lamellar fashion.

Proper fixation of the gland is essential for the demonstration of mammalian parafollicular cells. The secretory granules and laminated endoplasmic reticulum of these cells can be preserved only by glutaraldehyde fixation. Braunstein's and Stephens's report of human parafollicular cells is highly questionable because of their failure to demonstrate diffuse granulation and lamellar endoplasmic reticulum. It is not sufficient merely to demonstrate a parafollicular location and a few membrane bound granules, for many follicular cells fulfil these criteria. Furthermore, the dilated endoplasmic reticulum in their illustrations is characteristic of follicular, rather than parafollicular, cells. Our study illustrates that it

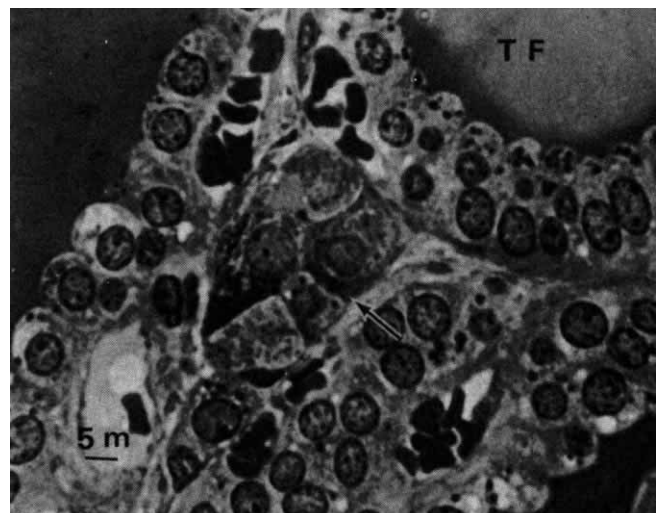


Fig. 1 Light micrograph of cluster of parafollicular cells (arrow) in relationship to thyroid follicle (TF) ($\times 900$).

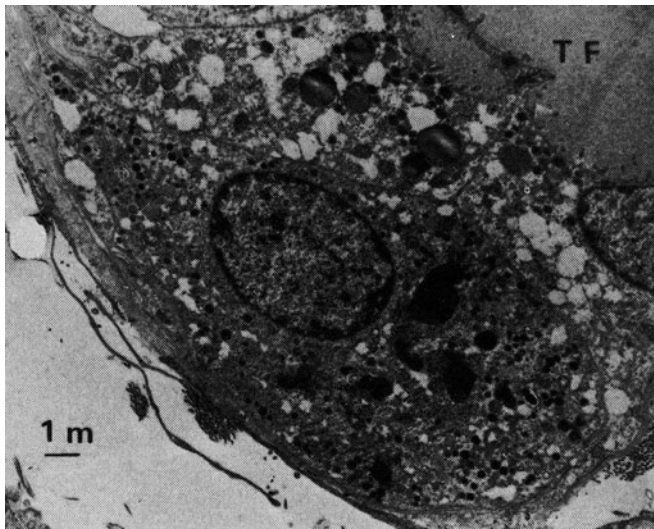


Fig. 2 Electron micrograph of parafollicular cells from same cluster as Fig. 1. Note diffusely distributed electron dense granules and laminated endoplasmic reticulum (arrow) ($\times 1,350$).

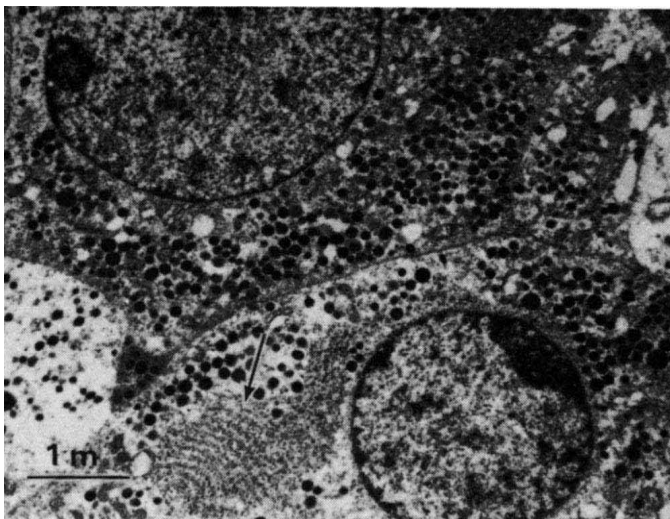


Fig. 3 Parafollicular cell in characteristic position between follicular cells and follicular basement membrane, not abutting on colloid (TF) ($\times 4,200$).

is not necessary to resort to silver impregnation techniques to demonstrate parafollicular cells by light microscopy. With proper fixation, the abundant and characteristic granules stain distinctly with such simple stains as toluidine blue.

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¹ Nonidez, J. F., *Amer. J. Anat.*, **49**, 479 (1932).

² Foster, G. V., Baghdiantz, A., Kumar, M. A., Slack, E., Soliman, H. A., and Macintyre, A. K., *Nature*, **202**, 1303 (1964).

³ Bauer, W. C., and Teitelbaum, S. L., *Lab. Invest.*, **15**, 323 (1966).

⁴ Bussolati, M., and Pearce, A. G. E., *J. Endocrinol.*, **37**, 205 (1967).

⁵ Haymovitis, A., and Rosen, J. F., *Endocrinology*, **81**, 993 (1967).

⁶ Mayer, J. S., and Abdel-Bari, W., *New Engl. J. Med.*, **278**, 523 (1968).

⁷ Braunstein, H., and Stephens, C. L., *Arch. Pathol.*, **86**, 659 (1968).

Transient Linkage Disequilibrium in *Drosophila*

THE results of some applications of biochemical techniques to population genetics apparently contradict certain basic tenets of evolutionary theory. Thus by the use of gel electrophoresis for soluble proteins, to estimate genetic variation within natural populations, levels of variability much greater than those predicted by the genetic load theory have been found¹⁻³. Second, the rate of allele substitution calculated from data on amino-acid substitutions in the primary structures of certain homologous proteins⁴ turns out to be two or three orders of magnitude greater than the productions of the genetic load theory for species of the appropriate evolutionary separation. Among the explanations so far proposed for these discrepancies are the selective neutrality *per se* of numerous isoalleles^{4,7-9}, frequency dependent selection^{10,11} and selectively maintained linkage disequilibria¹²⁻¹⁴. Although there have been many theoretical discussions of linkage disequilibria¹²⁻¹⁴, experimental evidence of their frequency and importance in natural population has been sparse^{15,16}. We now describe a transient linkage disequilibrium between two allozyme loci in a laboratory population of *Drosophila melanogaster*.

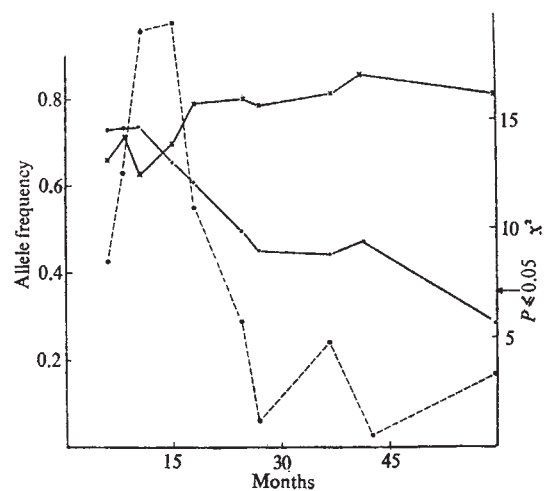


Fig. 1 Allele frequencies of *Acph-1^F* (x—x) and *Lap-D^F* (●—●) over a 60 month period. The broken line traces the χ^2 values to the ordinates on the right. The 0.05 level of significance, with 3 degrees of freedom, is indicated by the arrow. Standard errors of allele frequencies range from 0.059–0.078. Each point represents a single sample of approximately 200 chromosomes.

The population was founded in 1965 by 500 individuals of a stock recently collected from Commack, New York, and maintained in a Bennett type of cage¹⁷. The stock was polymorphic for two electrophoretic variants at each of two enzyme loci on chromosome III: *leucine aminopeptidase-D* (*Lap-D*, III-98.3)^{18,19} and *acid phosphatase-1* (*Acph-1*, III-101.1)²⁰ (locus recalculated from new *bv* locus at 102.7). Samples of approximately 400 eggs were collected several times during 5 yr. Because *Lap-D* activity is greatest in pupae, and to help measure chromosome frequencies with respect to the two loci as well as gene frequencies, we used the following sampling procedure. Adult males which developed from the egg samples were crossed individually in vials to tester stock females homozygous for fast alleles at both loci. Single pupae were taken from each of the mating vials and subjected to starch gel electrophoresis as before¹. The gels were sliced laterally and the tops were developed for leucine aminopeptidase and the bottoms for acid phosphatase. Because we know the mother's genotype, we could deduce from the two electrophoretic patterns of a single pupa the genotype of one chromosome from each parental male.

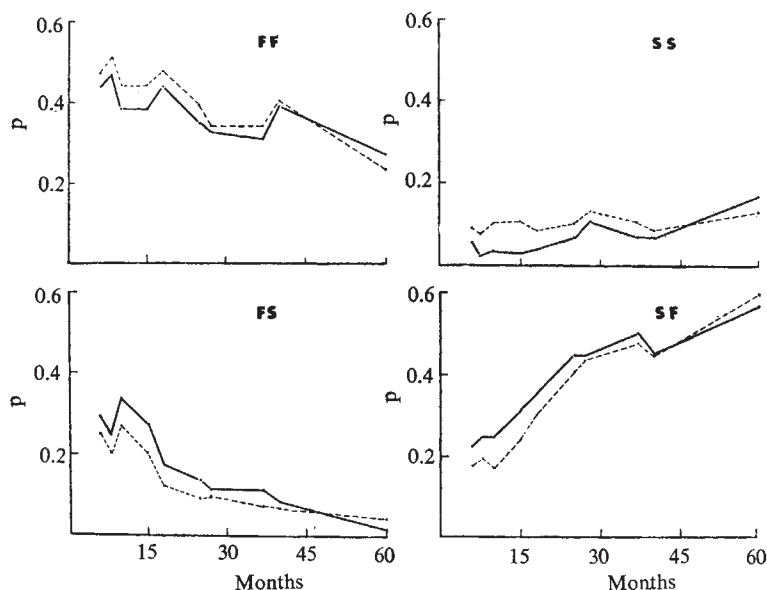


Fig. 2 Chromosome frequency distributions over 60 month period. —, Actual frequencies; - - -, the expected frequencies.

Each sample yielded three important measurements: allele frequency for both loci; chromosome frequencies; and divergence from linkage equilibrium. The disruption of linkage equilibrium could be estimated by a χ^2 test of the deviations of actual chromosome frequencies from expected chromosome frequencies. An analogous approach would be the calculation of the D value which measures deviation from 0 of the matrix of the 4 allele frequencies¹³.

Fig. 1 shows a plot of allele frequencies of one allele at each of the two loci during the experiment. Superimposed on this graph are the deviations from linkage equilibrium of each sample. The *Lap-D^F* allele decreased continually in the population while the *Acph-I^F* allele rapidly increased up to the eighteenth month, when a gene frequency equilibrium was established. During the most rapid changes in gene frequency (6–20 months) the distortion of linkage equilibrium was greatest. The χ^2 values during this time increased considerably above the significance level. During this period, there is a significant excess of the repulsion chromosomes, *Lap-D^F-Acph-I^S* (FS) and *Lap-D^S-Acph-I^F* (SF) over the coupling chromosomes (SS and FF) (see Fig. 2). Thus a coordinate change in gene frequency is accompanied by a sizable linkage disequilibrium over 18 months. Once gene frequency equilibrium is obtained, the linkage disequilibrium disappears.

Fig. 2 shows the changes in the four chromosome frequencies. The frequencies of the coupling chromosome change only slightly, but the repulsion chromosome frequencies change quite significantly; the SF chromosome increases continually, and the FS chromosome decreases rapidly during the linkage disequilibrium and then slightly during the later period. There seems to be a definite selective advantage associated with the SF chromosome, enough to generate a linkage disequilibrium initially until the constituent alleles are somewhat higher in frequency. This is followed by a continued but less pronounced advantage in the last few years of the experiment.

It is difficult to interpret these results. There is no reason to suppose that the products of the loci examined were subject to natural selection. *Acph-I* and *Lap-D* could simply be close chromosomal neighbours of the critical loci. Although the size of the disequilibrium is therefore unknown, the distance between the two loci is a minimum estimate of the distance involved. Second, because there were no control cages, there is no way to determine if selection is affecting one locus more than the other during the rapid adaptation to gene frequency equilibria.

The two loci are 2.8 map units apart, approximately 1% of the *Drosophila* genome. Laird and McCarthy²¹ have estimated that *Drosophila* contain on the order of 10^5 unique DNA

sequences the size of an average cistron. This means that the *Lap-D-Acph-I* region contains a minimum of 1,000 genes. If we assume that approximately 40% of the genome is polymorphic^{1–3} and that these loci are randomly distributed throughout the genome, then there are a minimum of 400 polymorphic loci in the region affected by the linkage disequilibrium. It is unlikely that all 400 of the polymorphic loci in any particular chromosome type would contain respectively identical alleles in each chromosome. Nevertheless, many of these chromosomes may well have nearly identical arrays of alleles at the polymorphic loci. Also, and probably most important, there is no requirement that the endemic alleles be selectively neutral themselves. The only requirement is that the action of selection on the outside loci be greater than that on the alleles of the loci between them.

The inside loci may therefore be “effectively neutral” merely as a result of their genetic environment, and make no contribution to the substitutional component of the population’s genetic load. This selection of blocks of genes may be quite important in periods of rapid adaptation, and although the disequilibrium we have found is transient, such situations might become permanent in certain cases.

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- ¹ O’Brien, S. J., and MacIntyre, R. J., *Amer. Nat.*, **103**, 97 (1969).
- ² Lewontin, R. C., and Hubby, J. L., *Genetics*, **54**, 595 (1966).
- ³ Selander, R. K., and Yang, S. Y., *Genetics*, **63**, 653 (1969).
- ⁴ Kimura, M., *Nature*, **217**, 624 (1968).
- ⁵ Haldane, J. B. S., *J. Genet.*, **55**, 511 (1957).
- ⁶ Kimura, M., *J. Genet.*, **57**, 21 (1960).
- ⁷ King, J. L., and Jukes, T. H., *Science*, **164**, 788 (1969).
- ⁸ Arnheim, N., and Taylor, C. E., *Nature*, **223**, 900 (1969).
- ⁹ Richmond, R. F., *Nature*, **225**, 1025 (1970).
- ¹⁰ Kojima, K., and Tobari, Y., *Genetics*, **63**, 639 (1969).
- ¹¹ Kojima, K., and Tobari, Y., *Genetics*, **61**, 201 (1969).
- ¹² Bodmer, W. F., and Felsenstein, J., *Genetics*, **57**, 237 (1967).
- ¹³ Lewontin, R. C., and Kojima, K., *Evolution*, **14**, 458 (1960).
- ¹⁴ Wills, C., Crenshaw, J., and Vitale, J., *Genetics*, **64**, 107 (1970).
- ¹⁵ Cannon, G. B., *Genetics*, **48**, 1201 (1963).
- ¹⁶ Rasmuson, M., Rasmuson, B., and Nilsson, L., *Hereditas*, **53**, 263 (1967).
- ¹⁷ Frydenberg, O., *Hereditas*, **48**, 83 (1962).
- ¹⁸ Beckman, L., and Johnson, F. M., *Hereditas*, **51**, 221 (1964).
- ¹⁹ Falke, E., and MacIntyre, R. J., *Dros. Info. Serv.*, **41**, 165 (1966).
- ²⁰ MacIntyre, R. J., *Genetics*, **53**, 461 (1966).
- ²¹ Laird, C. D., and McCarthy, B. J., *Genetics*, **63**, 865 (1969).

"Absolute" Pitch—a Bimensual Rhythm

DIFFERENT opinions have been expressed about the way in which some people are able to recognize and identify the pitch of a tone without the aid of an external reference tone. Few people possess "perfect pitch" and it is not known whether it is learned or inherited¹.

The preliminary results of the experiment reported here are based on one such person. I feel that this lack in generality has been compensated for by the intensive measurements and observations that could be carried out on this subject.

I first noticed the ability of the subject, a young woman, to identify the pitch of a tone 7 years ago. She was asked to comment on the frequencies which were produced in a loud-speaker by an audio oscillator. The frequencies were chosen in a random fashion either to correspond directly with the notes of a piano (tuned to A=440 Hz) or to be sharp or flat by approximately 1 Hz. In every case she was able to name correctly the piano note nearest to the oscillator frequency and say if it were sharp, flat or in tune. A later test showed that she satisfied Bachem's criterion for universal genuine absolute pitch².

Six years after the original test the subject was asked to sing what she thought was the note A (440 Hz) into a microphone which was connected through a power amplifier to a digital frequency meter. Measurements were repeated at intervals during the day and the experiment was continued over a period of 9 months in two intensive sessions. At various times during this period, measurements were made of the relative humidity, room temperature, atmospheric pressure and the subject's body temperature. Although no simple correlation was observed between the estimated note A and the first three quantities, there seemed to be a correlation with the observed variation in body temperature. Fig. 1 shows the well known variation in body temperature, taken first thing in the morning, of a woman (the subject) as a function of time. The figure also includes the plot of the estimated pitch A against time.

The subject's estimate of A fluctuated during the day. This fluctuation was assumed to be random and for Fig. 1 each frequency reading was taken as the mean over all readings during that day. The standard error in the mean for the frequency readings was ~ 1 Hz.

Fig. 1 has several interesting features. (1) There is a base line at 462 Hz. It is not clear why the subject's internal reference note should apparently have drifted from 440 Hz to 462 Hz in 6 years. (2) A fluctuation in the base line of approximate magnitude 20 Hz coincided with a change in the state of health of the subject and repeated itself, but with a different recovery time, 6 months later. (3) There is a bimensual modulation of the base line. This modulation was also observed in the second part of the experiment. Although no hormonal analysis was made during the experiment, the positions of the periodic dips in the base line, when taken in conjunction with the basal temperature curve and a knowledge of the personal behaviour of the subject, suggest that they are related to the fluctuations in hormone secretions³⁻⁵.

To test whether the variations in the recorded frequencies were due to a hormonal effect on the voice-producing apparatus, the subject was asked at various times during the experiment to tune an audio oscillator to her internal reference note A unaided by her voice. Although insufficient readings were taken to detect the bimensual rhythm by this means, the two methods were found to be in good agreement.

Although the results are limited in that they apply to one person, I feel that a further investigation of the phenomenon by psychologists and chronobiologists might be extremely fruitful.

The results raise some interesting questions. (1) Is the bimensual variation in pitch governed by the change in the electrical activity of the brain, which is itself thought to arise

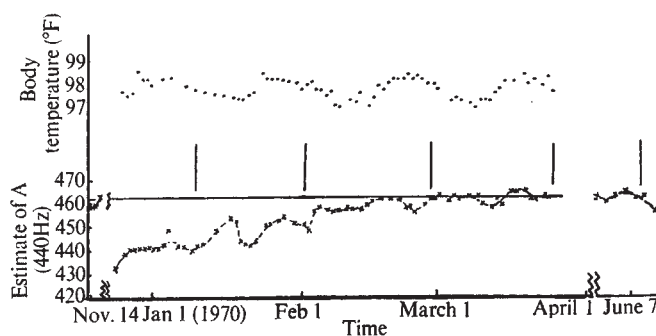


Fig. 1 The figure is a plot against time of both the early morning body temperature and the subject's estimate of the note A (440 Hz). The dashed line is the suggested bimensual rhythm. The vertical lines indicate the onset of menses. The continuous horizontal line represents the base line at 462 Hz.

from the effect of oestrogen on the sodium-potassium ratio^{5,6}, or is it due to the effect of the hormones on the cellular structure of the ear? (2) Why should illness affect the estimation of pitch in such a non-random way? (3) Can these bimensual perturbations of the time sense, if this is what they are, be responsible for some of the psychiatric symptoms of the premenstrual syndrome? The importance of the "sense of time" of the patient in some mental illnesses is well known and in 1969 Kopell *et al.*⁵ suggested the possibility that the subjective changes experienced during the premenstrual period might be related to a distortion of the basic time sense. It seems possible that the variation in absolute pitch through the month, and also the observed variations in the magnitude of the spread of the readings during the day through the month might be useful in an investigation of the premenstrual syndrome. The results of this and other work indicate that some people can compare external frequencies with an internal standard or standards. In an attempt to estimate the time keeping accuracy of this biological clock, the mean of all readings for a complete number of bimensual cycles was taken for two level portions of the base line. These were separated by a 6 month time interval. The two values were calculated to be: 462.1 ± 0.5 Hz, 461.8 ± 0.6 Hz. These results are indicative of a very accurate biological clock.

I thank my wife for being such a patient subject.

V. T. WYNN

Department of Physics,
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Received November 19, 1970.

¹ Cuddy, L. L., *J. Acoust. Soc. Amer.*, **43**, 1069 (1968).

² Bachem, A., *J. Acoust. Soc. Amer.*, **9**, 146 (1937).

³ Brown, G. B., Klopfer, A., and Loraine, J. A., *J. Endocrinol.*, **17**, 401 (1958).

⁴ Dalton, K., and Greene, R., *Brit. Med. J.*, 4818 (1953).

⁵ Kopell, B. S., Lunde, D. T., Clayton, R. B., and Moos, R. H., *J. Nerv. Ment. Dis.*, **148**, (2), 180 (1969).

⁶ Margerison, J. H., Anderson, W. McC., and Dawson, J., *EEG Clin. Neurophysiol.*, **17**, 540 (1964).

Nationality and Musicality used to test the Lamarckian Hypothesis

It is widely believed that certain nations have an inherited aptitude for music. The musical culture of the English in particular is considered sadly deficient by comparison with that of the Germans, the Slavs or the French¹. Britain provides a useful field for evaluating nationalist musical claims, for the Celts in Wales, Scotland and Ireland assert their musical superiority over their English conquerors (some say "oppressors"). On questions of national superiority the English remain silent, leaving time to establish the facts.

I have tried to test the assumption, accepted almost as an article of faith by the Welsh, that they have an above average

Table 1 Scores in Bentley's Measures of Musical Abilities

	Pitch Schools			Tunes Schools			Chords Schools			Rhythm Schools			Total Schools		
	A	B	C	A	B	C	A	B	C	A	B	C	A	B	C
(1)	12	8	15	7	6	1	10	9	3	4	9	5	33	32	24
(2)	5	7	3	4	7	0	6	8	10	4	4	2	19	26	15
(3)	13	8	6	9	5	7	10	10	16	3	6	7	35	29	36
(4)	6	7	8	1	4	3	1	3	3	2	4	2	10	18	16
(5)	7	11	12	3	8	0	8	8	8	6	8	5	24	35	25
(6)	11	8	7	6	2	2	3	7	6	5	4	6	25	21	21
(7)	13	18	14	7	9	8	10	9	13	8	9	7	38	45	42
(8)	5	14	13	3	5	4	4	11	14	6	5	9	18	35	40
(9)	12	16	16	8	9	2	11	12	10	10	7	6	41	44	34
(10)	11	17	9	9	10	0	13	15	9	8	6	4	41	48	22
(11)	14	5	8	2	7	5	8	8	9	8	6	3	32	26	25
(12)	15	14	11	9	10	3	11	8	9	9	5	9	44	37	32
(13)	9	15	15	5	9	6	11	11	8	2	9	2	27	44	31
(14)	11	17	18	9	8	8	14	9	8	4	7	9	38	41	43
(15)	15	14	11	10	5	4	10	12	11	9	6	7	44	37	33
(16)	15	10	10	9	3	5	13	3	11	9	5	8	46	21	34
(17)	8	14	14	6	9	9	6	12	14	4	8	8	24	43	45
(18)	11	12	13	10	10	9	13	9	12	9	8	9	43	39	43
Totals	193	215	203	117	126	76	162	164	174	110	116	108	582	621	559
Averages	10.7	11.9	11.3	6.5	7.0	4.2	9.0	9.1	9.6	6.1	6.4	6.0	32.3	34.5	31.1

Nos. 1-6, Intelligence below average. Nos. 7-12, Intelligence average. Nos. 13-18, Intelligence above average.

The first three in each group are boys, the rest are girls. Schools: (A) Welsh school. (B) School in anglicized part of Wales. (C) English school.

talent for music. Many people believe that the Welsh language is more "musical" than the English, that Wales is a country steeped in music and musical traditions and that these provide a "heritage of music" which is transmitted not only through the social environment but also through the reproductive process: Lamarck is pressed into service in the nationalist cause. An investigation was therefore instituted to test these views, to compare a Welsh speaking group of children with a pure English speaking group as regards measurable musical ability, with a third intermediate group from the Welsh border country known to be of mixed Welsh-English heredity. The children were chosen from three elementary schools—one in Bala (all Welsh speaking with all parents and grandparents known to have used Welsh as their original language), one in Wrexham (where the children were of mixed origin and bilingual) and one in Cambridge (all English, with parents and grandparents speaking only English). The groups were of equal age and matched in intelligence in terms of a standardized test of non-verbal intelligence. The socio-economic level of the three schools was ascertained to be the same, without any considerable variation within each school.

A random sample of nine boys and nine girls from each school was stratified by intelligence to ensure that each national group contained six children of average intelligence and six each above and below average intelligence. The three groups

were matched for age, sex, intelligence and socio-economic status: everything was done to ensure that they constituted representative samples of the three different schools. It could be argued that the randomization process coupled with the care taken to match the samples ensured that they were as near to being representative of Welsh, English and mixed children as possible with such a small sample. With such a sample it is possible to calculate the effects associated with national differences, but also those linked with sex, intelligence and the various interactions between these three factors.

Bentley's measures of musical abilities were used to assess the level of musical talent of each individual in the sample². For the Bala group, the test instructions were translated into Welsh and recorded on tape, for the original test has the instructions on the test record. Bentley's measures are scored in four parts to yield sub-scores for ability in pitch discrimination, recognition of tunes, chord analysis and memory for rhythm: a total score for "musical ability" is also obtained. Evidence of the validity and reliability of this test is presented elsewhere. The results of testing these three groups are shown in Table 1.

The analysis is summarized in Table 2 in terms of the total and average scores on the measures of musical abilities. Each of the sub-tests gives virtually the same result. The only significant differences are those which are associated with intelligence. The differences in score which are associated with sex and nationality differences are purely random deviations.

The average differences in scores of the three levels of intelligence are significantly bound up with each of the musical abilities at the probability level of less than one in a hundred: none of the other differences reach the 0.05 level.

We must therefore reject the hypothesis that the Welsh children are more musical than the English.

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Edmonton

CYRIL THOMAS

Cambridge Institute of Education

Received October 5, 1970.

¹ Rainbow, B., *The Land Without Music: Musical Education in England 1800-1860 and its Continental Antecedents* (Novello, London, 1967).

² McLeish, J., *Brit. J. Educ. Psychol.*, **38**, 201 (1968).

Table 2 Musical Ability, Nationality, Sex, Intelligence

		Average scores			Total group	
		IQ < 100	IQ 100-115	IQ > 115		
Boys	Welsh	29.0	32.3	36.3	32.56	34.3
	Mixed	29.0	41.3	40.7	37.00	
	English	25.0	38.7	35.7	33.22	
Girls	Welsh	19.7	39.0	37.7	32.11	34.5
	Mixed	24.7	37.0	34.3	32.00	
	English	20.7	26.3	40.7	29.22	
Mean score for IQ levels		24.7	35.9	42.6	34.4	
Boys	Welsh	32.56	37.00	33.22	34.3	
	Girls	32.11	32.00	29.22	34.5	
National differences		32.34	34.50	31.22	34.4	

CORRESPONDENCE

Brain Drain

SIR,—After eight years in North America, I have recently accepted a full-time academic appointment at a postgraduate medical institution in India. It was with considerable interest therefore that I read the article by Mr Ashok Parthasarathi (*Nature*, 230, 87; 1971).

Although Mr Parthasarathi has suggested comprehensive measures for the prevention and reversal of the brain drain, in my opinion he has not taken into consideration certain human aspects of the problem. I wish to point these out briefly.

I firmly believe that Indians in North America who do not intend to return fall into one or more of the following groups. (1) Probably the largest group is made up of those who after leaving India initially suffered the pangs of socio-cultural adjustment, but are now too comfortably rehabilitated to risk retracing their steps. (2) Those who have Western wives, a vast majority of whom will not

be able to adjust to the socio-cultural realities of Indian life. Equally difficult is the problem of Indian children born and brought up in a Western society when they have to make psycho-social adjustments in their "own" society. (3) The truly first-rate scientists to whom the facilities to pursue their ideas and the right intellectual atmosphere are far more important than the geographical location of their laboratory. (4) Those who believe that they can meet their economic obligations towards their family in India much more effectively by staying in North America. They also believe their children deserve a "better deal". (5) Religious minority groups from India who naturally prefer to suffer minority-disadvantages in an affluent society rather than in a poor society. (6) That undefinable group which habitually recites all the ills in the Indian society: overpopulation, corruption, language problems, poverty and so on. They either wish to stay away from these ills or wish them away.

The question therefore is, how do the

measures advocated by Mr Parthasarathi—laudable though they are—stand up against these real problems?

The measures he suggests to prevent uncontrolled brain drain appear too dictatorial in conception and are most likely to be misused.

Perhaps there is a moral in my personal experience. I came to the US in 1963 after attaining the highest levels of training then available in India. I was in the Government Service for at least 5 years. As an exchange visitor, US immigration did not permit me to stay more than 4 years. I emigrated to Canada in 1967 and did well. I am at present under no compulsion or obligation to return but am doing so of my own free will. That is the crux of the matter.

Yours faithfully,

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Obituary

Sir Lindor Brown



THE death of Sir Lindor Brown, FRS, on February 22, 1971, is a great loss to Hertford College and to physiology. G. L. Brown graduated in physiology

from Manchester University and later qualified in medicine in 1928. His inclinations, however, remained on the scientific side of medicine, and he joined B. A. McSwiney's department in Leeds as a lecturer in physiology. During his stay there he spent a brief sabbatical period in Oxford, where he was to return many years later as professor.

In 1934 he was invited by H. H. Dale to join his research group in the MRC laboratories at Hampstead. This move came at a crucial time in those laboratories. In the short space of four years between 1933 and 1936 Dale and his collaborators provided the experimental evidence for transmission by chemicals, rather than electric currents, of activity from nerves to the cells they innervate. To this work G. L. Brown contributed superb experimental flair allied to an astute intuitive ability to see how to obtain direct convincing evidence from simple experiments. When H. H. Dale retired in 1940, Brown became head of the physiology and pharmacology division and guided it through the difficult war-time period with outstanding success. In 1949 he became Jodrell professor of physiology at University College London, and there began his research work on the transmitter at sympathetic nerve endings which was to continue actively at Oxford

right up to his death. This represents his second major contribution to our understanding of the function of efferent nerves.

In 1946 he was elected to the Royal Society and became biological secretary in 1955. In 1960 he left University College to become Waynflete professor of physiology and Fellow of Magdalen College, Oxford. In 1967 he became principal of Hertford College, Oxford.

G. L. Brown's scientific work falls into three phases. In the first, an apprenticeship in B. A. McSwiney's department, in which he studied the complex effects from stimulation of autonomic nerves in the gut, he may well have been depressed by the difficulties of interpreting such experiments; certainly, after his brief period in Oxford (where he worked with Eccles on the effects of the vagus on the heart), his subsequent publications increasingly emphasized the advantages of electric measurement of response at a cellular level rather than the final response of the organ. His most important work was done in the Hampstead laboratories of H. H. Dale. There a brilliant group of investigators from several countries—Dale, Gaddum, Feldberg, Brown, Vogt and Bacq—in a very few years laid the experimental foundation for the theory of chemical transmission, and chemically

characterized all of the known efferent nerve fibres: preganglionic fibres, postganglionic sympathetic and parasympathetic fibres, the nerve fibres to the adrenal medulla and the efferent nerves to skeletal muscle. Even the exceptions, such as the cholinergic fibres to sweat glands, were defined. Brown worked both on transmission in sympathetic ganglia and at the skeletal neuro-muscular junction, but it was the latter topic for which he became best known. In all cases, the existence of acetylcholine, its disappearance after degenerative nerve section and its release by nerve stimulation or by potassium ions were shown. For skeletal muscle one important gap remained, the ability of acetylcholine to mimic the action of nerve stimulation. Contraction by acetylcholine could be produced in denervated muscle, but normally innervated tissue was insensitive and inconstant in response. Brown devised the technique of close arterial injection whereby the acetylcholine was introduced so close to the muscle that it could be delivered with little time for destruction in the blood. In this way constant responses to small doses of acetylcholine, equal in amplitude to the effect of nerve stimulation, were produced. The significance of other experimental work on neuro-muscular physiology done at that time has only recently been appreciated; for example, the species variations between avian and mammalian muscle, reflected in their

different sensitivity to modern blocking agents, or the importance of the calcium ion for the release of transmitter.

It was his interest in the nervous control of skeletal muscle which curiously enough led Brown to the problems of adrenergic transmission. With von Euler just before the war he had studied the potentiating effect of tetanic stimulation to a muscle nerve on the response to a single stimulus. After the war, further work on post-tetanic enhancement suggested that it might be due to an increased release of transmitter. In an attempt to demonstrate this increase the transmitter "sympathin" liberated from sympathetic nerve to the spleen was examined. Though this demonstration was never successful, an unexpected observation was that the drug phenoxybenzamine could increase the amount of sympathin appearing in the venous blood leaving the spleen after nerve stimulation. This finding has had important repercussions. Further experiments in his own and other laboratories have shown that sympathin is inactivated mainly by re-uptake into the nerves which liberate it, rather than destruction by enzymes as occurs with acetylcholine, and that this re-uptake is essential if the nerve stores are to be maintained during high neuronal activity.

Physiology is still in the happy position of having its affairs regulated in the main by its own active practitioners. In this

G. L. Brown set an example which will be stressful to emulate and almost impossible to excel. For 22 years he served the Physiological Society as committee member, secretary, editor or foreign secretary. Internationally, he served on the International Union of Physiological Sciences, eventually becoming its president. He was also a member of the Royal Danish Academy of Sciences and a foreign member of the Brazilian Academy of Science. He was a member of the Medical Research Council and of the Council for Scientific and Industrial Research. Soon after the end of his period as biological secretary of the Royal Society in 1963, he found himself serving on the Franks Commission of Enquiry into the working of the University of Oxford. From these activities, Oxford scientists generally and physiologists in particular have benefited. But to many he will be remembered most characteristically from the meetings of the Physiological Society—as entertaining after dinner as he was perceptive at the scientific meetings, his questions direct and to the essence of the experiment, seeking to measure its worth and the validity of the author's interpretation, but always with humour and sympathy, especially for the young. His many colleagues, and especially the many young physiologists he encouraged, will find it hard to believe that G. L., with his boundless energy, is no longer with us.

Announcements

University News

Professor O. Hood Phillips has been appointed pro-vice-chancellor and vice-principal of the **University of Birmingham**, in succession to **Professor F. W. Shotton**.

Professor A. L. Hodgkin has been appointed chancellor of the **University of Leicester**, in succession to **Lord Adrian**.

Miscellaneous

At the Stated Meeting of the **Royal Irish Academy**, held on March 16, **Dr V. C. Barry**, director of the laboratories of the Medical Research Council of Ireland, was elected president of the Academy, **Dr W. O'Sullivan** was elected treasurer and **Professor J. R. McConnell** was elected secretary. **Dr T. Walsh**, director of the Agricultural Institute, will be president of the Committee of Science; other members of the committee are **Dr P. K. Carroll**; **Dr W. Cocker**; **Dr J. N. R. Grainger**; **Dr D. J. Judge**; **Dr J. R. McConnell**; **Mr G. F. Mitchell**; **Dr T. Murphy**; **Dr F. J. O'Rourke**; **Dr E. M.**

Philbin; **Dr A. E. J. Went**. New members of the Academy, elected to the Section of Science, include **Dr C. H. Holland**, **Dr R. S. McElhinney**, **Dr D. G. O'Donovan** and **Dr M. C. Sexton**. **Professor V. Prelog** and **Professor R. L. M. Synge** were elected honorary members of the Academy.

ERRATUM. In the article "Specific Ion Electrode in the Determination of Urinary Fluoride" by **A. A. Cernik**, **J. A. Cooke** and **R. J. Hall** (*Nature*, **227**, 1260; 1970), the word "trisodium" on lines 3 and 17 of paragraph 3 should read "sodium".

International Meetings

May 6, **The Structures of Flexible Molecules**, London (**Dr J. F. Gibson**, The Chemical Society, Burlington House London W1V 0BN).

May 14–16, **Starting One's Own Business**, Fulmer Grange (Meetings Officer, The Institute of Physics and the Physical Society, 47 Belgrave Square, London SW1).

June 2–5, **Tooth Morphology**, Brussels (Service de Stomatologie, Hôpital Uni-

versitaire Saint-Pierre, Rue Haute 322, 1000 Bruxelles, Belgium).

July 14, **Glasshouse Crops Research Institute open day**, Littlehampton (Glasshouse Crops Research Institute, Worthing Road, Rustington, Littlehampton, Sussex).

August 2–20, **Computing as a Language of Physics**, Trieste (**A. M. Hamende**, International Centre for Theoretical Physics, Miramare, PO Box 586, 34100 Trieste, Italy).

August 5–11, **Informational Structures**, Aarhus, Denmark (**Professor N. O. Kjeldgaard**, **Professor K. A. Marker**, Department of Molecular Biology, University of Aarhus, 8000, Aarhus C, Denmark).

August 11–13, **Computer Controlled X-Ray and Neutron Diffraction Experiments**, Denver, Colorado (**Dr C. O. Ruud**, Metallurgy and Material Sciences Division, Denver Research Institute, University of Denver, Denver, Colorado 80210).

August 11–13, **The Application of High Resolution Solid State Detectors to X-ray Spectrometry—A Review**, Denver, Colorado (**Dr C. O. Ruud**, Metallurgy and Material Sciences Division, Denver Research Institute, University of Denver, Denver, Colorado 80210).

August 11–13, **Application of Computers in Electron Probe and X-ray Fluorescence Analysis**, Denver, Colorado (Dr C. O. Ruud, Metallurgy and Material Sciences Division, Denver Research Institute, University of Denver, Denver, Colorado 80210).

August 11–13, **Characteristics of Gas Flow Proportional Counters**, Denver, Colorado (Dr C. O. Ruud, Metallurgy and Material Sciences Division, Denver Research Institute, University of Denver, Denver, Colorado 80210).

August 11–13, **Detection of Single Ions by Pulse Counting**, Denver, Colorado (Dr C. O. Ruud, Metallurgy and Material Sciences Division, Denver Research Institute, University of Denver, Denver, Colorado 80210).

August 13–21, **Wolf-Rayet Stars**, Buenos Aires (Dr M. K. Vainu Bappu, Universidad Nacional de la Plata, Observatorio Astronomico, La Plata, Rep. Argentina).

August 13–26, **Science and Technology of Superconductivity**, Washington DC (School for Summer and Continuing Education, Georgetown University, Washington, DC 20007, USA).

August 16–28, **Electron Spin Relaxation in Liquids**, Spatind, Norway (Prof. L. T. Muus, Chemistry Dept., 140 Langelandsgade, 8000, Aarhus C, Denmark).

August 19–26, **Interactions between Land and Water**, Leningrad (Organizing Committee, Limnologorum Conventus XVIII, Laboratory of Limnology, Petrovskaya ulitsa 3-a, Leningrad 46, USSR).

August 23–28, **International Congress of Chemotherapy**, Prague (VIIth International Congress of Chemotherapy, Sokolska 31, Praha 2).

August 29–September 3, **Information Science**, Ljubljana (The Organizing Committee, International Conference on Information Science, PO Box 16271, Tel Aviv, Israel).

August 30–September 1, **Education in and History of Modern Astronomy**, New York (Dr Richard Berendzen, Department of Astronomy, Boston University, 725 Commonwealth Avenue, Boston, Massachusetts 02215, USA).

August, **Crystal Structure and Chemical Bonding**, Technological University, Twente (Dr A. Schuijff, Laboratory for Crystal Chemistry, University of Utrecht, Catharijnesingel 51, Utrecht, The Netherlands).

September 6–16, **Peaceful Uses of Atomic Energy**, Geneva (IAEA, Kärltnerring 11, PO Box 590, A-1011 Vienna, Austria).

September 6–21, **Transplantation Genetics of Primates**, Rijswijk (H. Balner, Radiobiological Institute TNO, Lange Kleiweg 151, Rijswijk ZH, The Netherlands).

September 11–15, **New Research and Development in Cosmetic Science**, Ham-

burg (Dr H. P. Fiedler, 6200 Wiesbaden, Lanzstrasse 4, Germany).

September 13–16, **Liquid Scintillation Counting**, Brighton (Mr M. A. Crook, Society for Analytical Chemistry, 9–10 Savile Row, London W1X 1AF).

September 14–16, **Quinone Chemistry**, Aberdeen (Dr L. Batt, Department of Chemistry, University of Aberdeen, Mes-ton Walk, Aberdeen AB9 2UE, Scotland).

September 14–16, **Solid State Devices**, Lancaster (Meetings Officer, The Institute of Physics and the Physical Society, 47 Belgrave Square, London SW1).

September 14–25, **Astronomy, Chemistry, Geology and Physics of the Moon** (NATO Advanced Study Institute), Patras (Professor C. L. Goudas, Department of Mechanics, University of Patras, Greece).

September 19–24, **The Role of Aquitards in Multiple Aquifer Systems**, Pacific Grove, California (Professor Paul A. Witherspoon, Department of Civil Engineering, 472 Davis Hall, University of California, Berkeley, California 94720, USA).

September 20–24, **European Thyroid Association**, Bern (Dr C. Beckers, MD, University of Louvain, Cliniques Universitaires St Pierre, Brusselsestraat 69, B-3000 Leuven, Belgium).

September 21–October 2, **Methods of Prospecting for Uranium Minerals** (NATO Advanced Study Institute), London and Cornwall (Dr Michael Davis, UK Atomic Energy Authority, 11 Charles II Street, London SW1).

September 23–24, **Data Processing and Display for Inspection Purposes**, Lancaster (Meetings Officer, The Institute of Physics and the Physical Society, 47 Belgrave Square, London SW1).

September 27–30, **Fracture Mechanics and Earthquake Source Mechanisms**, Aspen, Colorado (R. E. Riecker, Air Force Cambridge Research Laboratories (LWW), Bedford, Massachusetts 01730, USA).

September 28–October 1, **Centralized Control Systems**, Savoy Place, London (Manager, Conference Department, IEE, Savoy Place, London WC2R 0BL).

September 30, **Microbial Control in Pharmaceutical and Cosmetic Preparations**, Imperial College, South Kensington (Mr R. E. Marshall, The Pharmaceutical Society of Great Britain, 17 Bloomsbury Square, London WC1).

October 14, **Fibrous Structures in Biomedical Applications**, New York City (Mr G. A. M. Butterworth, Fabric Research Labs., Inc., 1000 Providence Highway, Dedham, Massachusetts 02026, USA).

October 18–22, **Eurocon**, Lausanne (Eurocon 71 Office, Swiss Federal Institute of Technology, Lausanne, 24 Chemin de Bellerive, CH-1007, Lausanne, Switzerland).

British Diary

Monday, April 5

Design Problems of Railcar Final Drives (5.30 p.m.) Mr V. Lehel, Institution of Mechanical Engineers, at 1 Birdcage Walk, London SW1.

Elementary Particle Physics (three-day conference) Institute of Physics and the Physical Society, at the University of Lancaster.

Pollution in Context (6 p.m.) Dr Martin W. Holdgate, Royal Society of Arts, at John Adam Street, Adelphi, London WC2. (Last of four Cantor Lectures on "Problems of Pollution".)

Testing Methods and Techniques (5.30 p.m. discussion) Institution of Electrical Engineers, at Savoy Place, London WC2.

Third National Atomic and Molecular Physics Conference (four days) Institute of Physics and the Physical Society, at the University of York.

Tuesday, April 6

Business, Society and the Professional Engineer (5.30 p.m.) Mr B. M. Maskell, Institution of Electrical Engineers, at Savoy Place, London WC2.

Electrical Control of Air Conditioning in Buildings (5.30 p.m. discussion) Institution of Electrical Engineers, at Savoy Place, London WC2.

Evaluation Testing of Vehicles (6 p.m.) Institution of Mechanical Engineers, at 1 Birdcage Walk, London SW1.

Scientific Papers and Annual General Meeting, Chemical Society, Protein Group, in the Physiology Lecture Theatre, Downing Street, Cambridge.

Wednesday, April 7

Control of Food from Farm to Table (10.30 a.m. symposium) Society of Chemical Industry, Microbiology Group, jointly with the Food Group and the Royal Society of Health, at the National College of Food Technology, St George's Avenue, Weybridge, Surrey.

Dielectrics at Very Low Temperatures (2 p.m. discussion) Institution of Electrical Engineers, at Savoy Place, London WC2.

Forensic Chemistry (6.30 p.m.) Mr R. L. Williams, Society for Analytical Chemistry, in the Scientific Societies Lecture Theatre, 23 Savile Row, London W1.

High Voltage Transmission Development (7.30 p.m.) Mr D. M. Cherry, Institution of Electrical Engineers, at CERL, Cleeve Road, Leatherhead, Surrey.

Standard Conditions for the Assessment of Colour (3 p.m.) Mr C. R. Bullett, Mr J. E. Cotton, Mr I. Glasman and Mr D. G. C. Thornley, Colour Group (Great Britain), in the Mechanical Engineering Department, Imperial College, Exhibition Road, London SW7.

The Expansion of the Fisheries: The Application of Mechanical Power in Man's Oldest Industry (6 p.m.) Mr G. C. Eddie, Institution of Mechanical Engineers, at 1 Birdcage Walk, London SW1. (James Clayton Lecture.)

The Problems of Painting Timber (4.30 p.m.) Mr G. W. Rothwell, Oil and Colour Chemists' Association, at the Manchester Literary and Philosophical Society, 36 George Street, Manchester 1. (Student Lecture.)

The Use of Telecommunications in Meteorology (5.30 p.m.) Mr A. A. Worthington, Institution of Electrical Engineers, at Savoy Place, London WC2.

Reports and Publications

not included in the Monthly Books Supplement

Great Britain and Ireland

Philosophical Transactions of the Royal Society of London. B: Biological Sciences. Vol. 259, No. 833 (11 February 1971). The Corpora Pedunculata of *Sphinx Ligustri* L. and other Lepidoptera: an Anatomical Study. By Lucy Pearson. Pp. 477-516 + plates 31-35. £1.50; \$3.90. Vol. 259, No. 834 (11 February 1971). Observations on the Structure of the Small Intestine in Foetal, Neo-Natal and Suckling Pigs. By R. N. Hardy, A. R. Hockaday and R. L. Tapp. Pp. 517-531 + plates 36-42. £0.95; \$2.45. (London: The Royal Society, 1971.) [252]
A Guide to the Literature on Spectral Data. By A. Clarke. Pp. 11. (London: National Reference Library of Science and Invention, 1971.) [13]
National Economic Development Office. Qualified Manpower in the Electronics Industry—a Preliminary Report. (Report of the Electronics EDC Working Group on Scientific and Technological Manpower.) Pp. xii + 44. (London: National Economic Development Office, 1971.) [23]
A Proposed Programme of Support for the Physics of Surfaces. Pp. 19. (London: Science Research Council, 1971.) *gratis*. [53]
Philosophical Transactions of the Royal Society of London. B: Biological Sciences. Vol. 260, No. 836 (4 March 1971). A Discussion on the Results of the Royal Society Expedition to Aldabra 1967-68. Organized by T. S. Westoll and D. R. Stoddart. Pp. 1-654. (London: The Royal Society, 1971.) £19; \$50. [83]
University of Essex Vice-Chancellor's Report 1969/1970. Pp. 45 + xxxiii. (Wivenhoe Park, Colchester: University of Essex, 1971.) [93]
Department of Education and Science. Education Survey No. 9: Diagnostic and Assessment Units for Young Handicapped Children. Pp. iii-19. (London: HMSO, 1970.) 20p net. [93]
Imperial College. Annual Report 1969/1970. Pp. iv+98. Annual Accounts 1969/1970. Pp. 31. (Lon-

don: Imperial College of Science and Technology, University of London, 1971.) [93]
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